PERIODICAL ROOM
GENERAL LIBRARY
UNIV. OF MICH.

JAN 11 1932

SCIENCE

NEW SERIES
VOL. 75, No. 1932

FRIDAY, JANUARY 8, 1932

SUBSCRIPTION, $6.00
SINGLE COPIES, 0.15

CLAYTON—Colloid Aspects of Food Chemistry

64 Illustrations. 571 Pages. Cloth, $8.00 Postpaid.

By W. CLAYTON, F.I.C., Chief Chemist and Bacteriologist to Messrs. Crosse and Blackwell, Ltd. (London).

CLOWES and COLEMAN—Quantitative Chemical Analysis *13th Edition*

133 Illustrations. 605 Pages. Cloth, $5.75 Postpaid.

By F. CLOWES, D.Sc., London, and J. B. Coleman, A.R.C.S., Dublin. Revised by D. Stockdale, Ph.D., Cambridge, Fellow of King's College, and J. Dexter, M.A., Cambridge, Formerly Scholar of King's College.

GLASSTONE—Recent Advances in Physical Chemistry

32 Illustrations. 470 Pages. Cloth, $3.50 Postpaid.

By S. GLASSTONE, F.I.C., Lecturer in Physical Chemistry at the University of Sheffield.

ROBBINS—Botany of Crop Plants *3rd Edition*

269 Illustrations. 639 Pages. Cloth, $4.00 Postpaid.

By W. W. ROBBINS, Professor of Botany, College of Agriculture, University of California.

PISTON—Meteorology

94 Illustrations. 187 Pages. Cloth, $2.50 Postpaid.

By D. S. PISTON, S.B., Assistant Professor of Physics, University of Maine, Orono, Maine.

JENSEN—The Chemistry, Flavoring and Manufacture of Chocolate Confectionery and Cocoa

23 Illustrations. 406 Pages. Cloth, $7.50 Postpaid.

By H. R. JENSEN, M.Sc. (Liverpool), F.I.C., Formerly Chairman of the Consultative Panel British Research Association for Cocoa, etc.

P. Blakiston's Son & Co. Inc.

1012 WALNUT STREET **PHILADELPHIA**

Entered as second-class matter July 18, 1923, at the Post Office at Lancaster, Pa., under the Act of March 3, 1879.

Ethnogeography of the Yuma Indians

Handsomely and profusely illustrated

By C. DARYLL FORDE

Relatively undisturbed in Spanish and Mexican times, the Yuma maintained their original culture almost unchanged until the fifties of the last century when the establishment of the caravan trail to southern California terminated their freedom.

Mr. Forde has given in readable form a balanced picture of a vanished remarkable culture.

Price, postpaid $2.25

Catalogue of Publications sent free for the asking

University of California Press Berkeley

MERCK'S INDEX

an Encyclopedia of Drugs and Chemicals

"One of the best handbooks for the advanced student and analyst that was ever compiled. It might well be termed a desk edition of Beilstein, a companion for the expert . . . The references to chemical substances note the atomic arrangement, contain brief descriptions of the properties . . . Special attention should be called to a list of indicators for determining pH concentrations; a short section on qualitative reactions of acids, bases, metals, and salts; reactions of the more important alkaloids and glucosides; and a list of atomic weights brought down to 1929."—*Industrial and Engineering Chemistry.*

4th edition, 1930, 585 pages. 6½ x 9¼ inches. Fabrikoid leather. $5.00; discount of 50 per cent ($2.50) to members of and those associated with the chemical and allied professions.

Send remittance, with identification of sender, to

MERCK & CO. INC.

RAHWAY N. J.

THE WISTAR INSTITUTE BIBLIOGRAPHIC SERVICE

A condensed summary of each paper appearing in the journals listed below is issued prior to publication of the article in full. This distribution of abstracts enables a biologist to familiarize himself with *contemporary research* in a minimum of time, and to select the articles which he wishes to purchase in reprint form. To receive these abstracts he may subscribe to the Advance Abstract Sheets or to the Bibliographic Service Cards.

The Advance Abstract Sheets are issued twice a month, each sheet containing ten or more abstracts, together with the price per reprint. Subscription rate is $3.00 per year.

The cards also are issued twice a month. In addition to the abstracts the cards provide subject headings and complete bibliographic reference. The cards are convenient for filing and library records. Price, $5.00 per year.

At regular intervals the abstracts are assembled and published in book form with complete authors' and analytical subject indices. Price, $5.00 per volume.

Journal of Morphology and Physiology
The Journal of Comparative Neurology
The American Journal of Anatomy
The Anatomical Record
The Journal of Experimental Zoology
American Anatomical Memoirs
American Journal of Physical Anthropology
Folia Anatomica Japonica (Tokyo, Japan)
Physiological Zoology (Chicago, Illinois)
Stain Technology (Geneva, New York)
Ecological Monographs (Durham, North Carolina)

The Wistar Institute of Anatomy and Biology

Thirty-sixth Street and Woodland Avenue
Philadelphia, Pa., U. S. A.

If you teach

Anatomy—Zoology—Botany

The following Catalogs will gladly be sent upon request.

- ☐ No. 50—Botanical Models and Preparations.
- ☐ No. 51—Zoological Life History Development Preparations, with section devoted to insect development and habitat.
- ☐ No. 52—Comparative Anatomy, Models and Preparations.
- ☐ No. 53—Osteology, Skeletons, Skulls and other Bone Preparations — Human and Zoological.
- ☐ No. 54—Charts, Human Anatomy, Physiology,
- ☐ No. 55—Promi and Promar Microscopic Projection and Drawing Apparatus.
- ☐ No. 56—SPALTEHOLZ Transparent Preparations—Human and Zoological.
- ☐ No. 57—Anatomical Models. Obstetrical Phantoms.
- ☐ No. 58—Zoological Dissections.
- ☐ No. 59—Microscopic Prepared Slides.
- ☐ No. 60—Lantern Slides.
- ☐ No. 62—New Biological Teaching Material.
- ☐ Kappers Comparative Neurology Charts.
- ☐ Strumpell and Jakob Neurological Charts.

Visit our New and Greatly Enlarged Display Rooms and Museum

CLAY-ADAMS COMPANY
117–119 East 24th Street, New York

SCIENCE

VOL. 75 FRIDAY, JANUARY 8, 1932 No. 1932

The American Association for the Advancement of Science:
Astronomy's Contribution to the Stream of Human Thought: PRESIDENT D. W. MOREHOUSE 27

The Physiology of Consciousness: PROFESSOR EDWIN G. BORING 32

Scientific Events:
The Master's Degree for Postgraduate Students in the Medical School of Columbia University; Further Investigation of Cosmic Rays; Presentation of The Perkin Medal to Dr. Burgess; Officers of the American Association for the Advancement of Science 39

Scientific Notes and News 42

Discussion:
A Theory of Disinfection: DR. M. L. ISAACS. *The Cotton Root Rot Fungus Indigenous in Arizona Deserts:* C. J. KING, CLAUDE HOPE and E. D. EATON. *Is Evolution only a Detail in the Dynamics of Populations?:* DR. JOHN STANLEY. *The Growth of Stalactites:* DR. GRAGG RICHARDS 46

Special Correspondence:
Field Anthropology in Australia: J. B. CLELAND 50

Scientific Apparatus and Laboratory Methods:
Concentrating Paramecium and Rotifers without Centrifuging: PROFESSOR EDGAR P. JONES. *A Simple Photomicrographic Apparatus for Obtaining Depth or Focus:* BRANDON H. GROVE 52

Special Articles:
Chemical Studies on Toad Poisons, IV, Bufagin and Cinobufagin: DR. H. JENSEN. *Cryolysis of Lyophilic Colloids, and its Bearing on the Mechanism of Enzyme Action:* DR. F. F. NORD and O. M. VON RANKE-ABONYI. *Sex Differences in the Study of General Science:* VICTOR C. SMITH. *Progress in Western Duck Sickness Studies:* E. R. KALMBACH 53

Science News 10

SCIENCE: A Weekly Journal devoted to the Advancement of Science, edited by J. MCKEEN CATTELL and published every Friday by

THE SCIENCE PRESS

New York City: Grand Central Terminal
Lancaster, Pa. Garrison, N. Y.
Annual Subscription, $6.00. Single Copies, 15 Cts.

SCIENCE is the official organ of the American Association for the Advancement of Science. Information regarding membership in the Association may be secured from the office of the permanent secretary, in the Smithsonian Institution Building, Washington, D. C.

ASTRONOMY'S CONTRIBUTION TO THE STREAM OF HUMAN THOUGHT[1]

By President D. W. MOREHOUSE

DRAKE UNIVERSITY

Contemplated as one grand whole, Astronomy is the most beautiful monument of the human mind, the noblest record of its intelligence.—*Laplace.*

THE development of human thought frequently has been compared with the formation of a great river. No single source can be named as the origin. Numerous streams from wholly different sources flowing in diametrically opposite directions converge to give it being. Not infrequently it is difficult to determine which is the main stream and which is the tributary.

The changes in its course can not be detected from up-stream. It turns back upon itself at the most unexpected moment and winds in and out among barriers which for the moment seem absolutely insurmountable. As it widens, it becomes shallow and its banks less distinct until new accretions cut deep gorges in the rocks of ignorance. Spring floods, too, have their analogy in the stream of thought. Samuel Butler's warning in Erewhon, "Woe to him who tampers with the banks while the flood is flowing," has been exemplified too frequently in astronomy.

[1] Address of the retiring vice-president and chairman of Section D—Astronomy, American Association for the Advancement of Science, New Orleans, December, 1931.

COSMOLOGY

As the various races emerge from their primitive concepts of cosmology, we find some far in advance of others. For example, the Hebrews and the Greeks have been held up to us from time immemorial as the greatest peoples of their time. The Hebrews excelled in their conception of the things of the spirit, and the Greeks in art, science and philosophy. The Hebrews, having little interest in science, disentangled religion from pseudo-science and crude materialism. The Greeks, with their transcendent genius, founded

science on actual observations and based their theories on facts.

Among the names of antiquity, Thales, Pythagoras, Menton, Aristotle and Plato, one Eudoxus stands out above all others. In point of time, Eudoxus lived 409–356 B. C. He, therefore, touched the life of Plato in his later years and that of Aristotle in his early days. Eudoxus is considered the first inductive thinker of his time. Berry says, "He may be regarded as representative of the transition from speculative to scientific astronomy, and much of his work was taken bodily by Aristotle, who, in common with other philosophers of his time, understood the influence of the concept of the physical world upon the thinking of the times.

Claudius Ptolemaeus

(100–170 A. D.)

The next important addition to the development of thought, after the coming of Christ, was started by Claudius Ptolemaeus, commonly known as Ptolemy. He was the propagator, if not the author, of the first cosmic theory of the universe, popularly known as the geocentric theory. The influence of this man is shown in the greatest work of antiquity, "The Almagest." If any one will wade through its thirteen books, he will find in them the cosmology and the cosmogony of his age. He will not only find the concepts of life, but also the ideas of eternity. There is an air of finality to this book which is not found in any other work of this time. It seemed to predict that the days of science had come to an end. As MacPherson points out:

> The Hellenic culture had largely exhausted itself and an air of hopelessness and futility had settled over the world. The Stoics rather concerned themselves with problems of conduct than with questions concerning the natural world, while the early Christians, expecting the early return of Christ, did not busy themselves with the affairs of this world. Accordingly, slowly but surely, science and philosophy alike seemed to die out.

During the next fourteen centuries, the Arabs, who were largely imitators and commenters rather than investigators, carried on the slender stream of scientific thought.

Nicolaus Copernicus

(1473–1543)

> Every stimulus to the intelligence naturally brings with it a tendency toward inquiry into opinions received through tradition and based on some great authority.

Toward the end of the fifteenth century, astronomy again added a new stream to human thought through the labors of the celebrated Polish astronomer, Copernicus, of whom Thomas Digges wrote:

> But in this our age, one rare witte (seeing the continuall errors that from time to time more and more continually have been discovered, besides the infinite absurdities in their Theoricks, which they have been forced to admit that would not confesse any Mobilitie in the ball of the Earth) hath by long studye, paynfull practise, and rare invention delivered a new Theorick or Model of the world, shewing that the Earth resteth not in the Center of the whole world or globe of elements, which encircled and enclosed in the Moone's orbit, and together with the whole globe of mortality is carried yearly round about the Sunne, which like a king in the middest of all, rayneth and giveth laws of motion to all the rest, spaerically dispersing his glorious beames of light through all this sacred coelestiall Temple.

The life of Copernicus was quite uneventful. He has been dubbed by nearly every writer as a plodder, and yet, no other one thing has so completely changed the trend of human thought as his bold statement that the earth is not the center of the solar system. From the earliest time, the geocentric idea of the universe had held sway. It is quite true that here and there a few lone figures suggested the possibility of another concept, but they were quickly stifled, and the earth and man restored to their position—the end and aim of creation.

One can hardly imagine the profound influence on the thought of the time of Copernicus' great book "The Revolution of the Celestial Bodies," setting forth the new order of things. To some it may seem a pity, to others, a blessing that he did not survive the publication of his book in 1543. At first, it was treated with the utmost contempt by authorities and with furious vindictiveness by theologians. We must remember that at this time the Reformation was at its height; Protestantism and Catholicism were in a titanic struggle with each other; Luther had pinned his theses to the church door, and yet that great Protestant was among the first and most vehement to denounce the Copernican system. He referred to Copernicus as "an upstart astrologer who strove to show that the earth revolved." "This fool," said Luther, "wishes to reverse the entire science of astronomy, but sacred scripture tells us that 'Joshua commanded the sun to stand still' and not the earth." Says Andrew D. White in "A History of the Warfare of Science with Theology in Christendom":

> While Lutheranism was thus condemning the theory of the earth's movement, Calvin in his "Commentary on Genesis" was condemning all who asserted that the earth is not at the center of the universe. He clinched the matter by the usual reference to the first verse of the ninety-third Psalm, and asked, "Who will venture to place the authority of Copernicus above that of the Holy Scripture?"

Suggestions were heard on every side that such views should be forcibly repressed, and some of its advocates, for example, Bruno, were condemned to death and burned at the stake in 1600. As history records, the second martyr of the Jesuits was harassed and persecuted solely for his adherence to the Copernican system.

> The greatness of Copernicus, [quoting from MacPherson] is not to be measured by what he did, but by what he made possible. A vast extent of the universe was revealed, and so the philosophic and theological outlook was vitally affected by the new system. While the scholastics were vainly trying to refute Copernicanism and the theologians were engaged in hurling anathemas at the heads of its supporters, the new system of the world was being established upon a firm scientific basis.

THE CONCEPT OF DYNAMICS

> Bacon pointed out at a distance the road to true philosophy: Galileo both pointed it out to others and made himself considerable advances in it.—*David Hume.*

The century following the death of Copernicus produced three great men of remarkable genius, Tycho Brahe (1546–1601), Kepler (1571–1630) and Galileo (1564–1642), an observer, a mathematician and a physicist. Kepler, who devoted a lifetime to the discovery of the three laws of planetary motion, which bear his name, did not conceive of their dynamical explanation. MacMillan says:

> He was content to ascribe them to the intelligence of an angel who guided the planets in their courses. His was the age of spirits and Kepler's interpretation of uniformities was Animistic. The foundations of dynamics came only with the genius of Galileo, who had little liking for the conceptions of Animism. His *induction* that the natural state of a body was uniform motion in a straight line, and that a departure from that state was due only to force was one of those great breaks with the Past which occur at rare intervals in human history and which have raised the race of men to its present intellectual level. Galileo initiated a new age, the one in which we live, the age of dynamics.

It was the work of Galileo, in his discovery of the revolution and phases of planets, that completely confirmed the Copernican doctrine. Father Inchofer of the Jesuits in denouncing him declared:

> The opinion of the earth's motion is of all heresy the most abominable, the most pernicious, the most scandalous. The immovability of the earth is thrice sacred. Argument against the immortality of the soul, the existence of God, and the incarnation should be tolerated sooner than the argument to prove that the earth moves.

Thus the controversy went on. The very thought of the earth as no longer the center of the universe was the real crux in the situation. If the Copernican system was true, then the entire outlook of human thought was changed, and the old, time-honored cosmology must be discarded. As the earth had lost its position among the heavenly bodies, so man would necessarily lose his supremacy in creation, and endless problems presented themselves.

CONCEPTS OF MECHANICS

Sir Isaac Newton—The Great Mechanist
(1642–1727)

> Nature and Nature's laws lay hid in night
> God said, "Let Newton be" and all was light.

Newton completed the work of these men and established the fundamental principles of Mechanics in the "Principia" which, as Laplace has declared, "has a preeminence over all productions of the human intellect." Says Andrew D. White:

> There came, one after another, five of the greatest men our race has produced—Copernicus, Kepler, Galileo, Descartes and Newton—and when their work was done, the old theological concept of the universe was gone. "The spacious firmament on high"—"The crystalline spheres"—"The Almighty enthroned upon the circle of the heavens" and with His own hands or with angels as his agents keeping the sun, moon and planets in motion for the benefit of the earth, opening and closing "the windows of heaven," letting down upon the earth "the waters above the firmament," "setting His bow in the cloud," "handing out signs and wonders," hurling comets, casting forth lightning to scare the wicked, and shaking the earth in His wrath: all this had disappeared.

The Newtonian theory was the natural outgrowth of the Copernican theory and had no less influence upon human thought. The old idea of celestial and terrestrial differences had been positively refuted. The solar system had been discovered. It was now clear that the fixed and law-abiding order of nature prevails not only upon this earth but also throughout the universe. Thus we have the beginning of mechanism —the whole universe reduced to a machine.

Quite contrary to the reception of the Copernican system, this thought was quickly grasped by the theologians. They had here from a profound scientist just what they had been looking for—a perfect system ruled over by a carpenter God. For Newton, in his perfect order of things, proves the existence of a God; an idea which drew from Leibnitz the criticism that "Newton had compared the universe to a clock which required the constant interference of the clockmaker."

Here we find a splendid basis for Deism. Many of the great men of England seized upon this idea and appropriated it to their uses. Through the writings of Ferguson, Paley and Thomas Dick, the mechanistic conception of the universe had a profound in-

fluence on human thought and prepared the way for Deism by commending it to the rational faculties. It was Laplace who cried out in protest to this great wave, "I have no need of the hypothesis of a God."

Sir William Herschel, The Artist
(1738–1822)

Coelorum perrupit claustra—*Herschel's Epitaph.*

Was it a dream?—that crowded concert-room
In Bath; that sea of ruffles and laced coats;
And William Herschel, in his powdered wig,
Waiting upon the platform, to conduct
His choir and Linley's orchestra?
He stood
Tapping his music-rest; lost in his own thoughts.
—*Alfred Noyes.*

The century that intervened between Newton and Herschel was probably more productive from the standpoint of human knowledge than any other like period. More great scientists and mathematicians arose during this interval than in any other corresponding time in human history. Time will not permit me to speak of the work of such men as Halley, Clairaut, Bradley, Euler, Lagrange, Laplace and Gauss.

They developed the greatest science ever conceived by the human mind, celestial mechanics. In the words of MacMillan, "They erected a monument to the human intellect that can never be forgotten as long as the mathematical faculties of men are active."

Undoubtedly these men lost much of their luster by suffering comparison with Newton. Stars, they were, of the first magnitude, but you can not see a star when the sun is shining.

Such was the condition of the astronomical world when the itinerant musician, Frederick William Herschel, made his way to Britannia: The sole aim of astronomers was the verification of the Newtonian theory, and human thought was completely centered on the solar system. The stars were merely guide-posts from which to take measurements and with which to compare results. What a discouraging situation for an artist! But Herschel was equal to the task which confronted him. By his artistry he transformed the humble guide-posts into monuments of surpassing splendor, by demonstrating what other observers had suggested as probable, "The essential kinship of the sun with the stars." By his observation of double stars, he extended the law of gravitation to the sidereal universe. As MacPherson points out:

> Copernicus had shown that the earth, so far from being the center of the universe, was but one planet among others in ceaseless revolution around the sun. Herschel now proved that the sun itself was not the central body, but was merely one star among others in ceaseless motion through the depths of space.

Newton had shown that the solar system was subject to one law, and Herschel demonstrated that this same law was applicable to the stellar system.

Thus we have the beginning of a new concept, cosmology, or the extension of the universe to a sidereal system. This required new postulates which had never been conceived, let alone proved, and endless observations which had not been made. As before pointed out, everything had been confined to the solar system. Now we must have a new type of observations and an expansion of thought.

The sky was vast and the equipment at hand was meager, but Herschel met both challenges, constructed his own instruments and made a careful survey of the entire sky in duplicate. As a result of his observations, he first formulated the disc or grindstone theory of the stellar system, which he later abandoned, but left his vast store of data to his successors. He was convinced, however, that the Galaxy or Milky Way is a greater system of which the solar system is a mere atom. His theory "that star clusters and nebulae formed universes external to our galaxy" implied an immense extension of the universe not only in space, but in time. As he said, "I have looked farther into space than ever human being did before me," adding more hypothetically, "I have observed stars of which the light, it can be proved, must take two millions of years to reach the earth." What an influence such ideas must have had on contemporary thought is suggested by Horace Walpole in his well-known expression, "One's imagination cracks."

Cosmogony

> The greater the sphere of our knowledge, the larger is the surface of its contact with the infinity of our ignorance.—*Anonymous.*

The ancients generally did not clearly distinguish between cosmology and cosmogony. "There had been implanted," says Andrew D. White, "along through the ages, germs of another growth in human thinking; some of them even as early as the Babylonian period. In the Assyrian inscriptions we find recorded the Chaldeo-Babylonian idea of an evolution of the universe out of the primeval flood or great deep." This thought was adopted by their neighbors, the Hebrews, but was soon stifled by the more powerful influence of their inherited doctrine. The Ionian school developed the idea more clearly. Anaximander, for instance, conceived of the visible universe as the result of evolution, and Aristotle carried it to a point which approached modern views. Notwithstanding the work of these men, the idea of creation in six literal days predominated in the minds of the masses for hundreds of years.

Probably the first great factor that influenced the church to accept these new doctrines was the work

of Ralph Cudworth in his "Intellectual System of the Universe," published in 1698. He argued most effectively against the prevailing mechanical theory and set forth the idea of a divine imminence in both theology and science. He says:

> Nevertheless, the substance or matter out of which the world was made was not itself made but always ready at hand, and subject to the artificer, to be ordered and disposed by him. For the making of the world was not the production of it out of nothing, but out of an antecedent, bad and disorderly state, like the making of a house, garment or statue.

It seems extremely difficult to locate the first idea of a nebular hypothesis. The Scottish astronomer, James Ferguson, strongly suggested the idea in the following sentence:

> In the beginning God brought all the particles of matter into being in those parts of open space where the sun and planets were to be found, and endowed each particle with an active power by which these neighboring and at first detached particles would in time come together in their respective parts of space and would form the different parts of the solar system.

History gives the credit of the nebular hypothesis to Wright, Kant, Laplace and Herschel. The first two developed it theoretically and from a deductive point of view. They postulated nebulae out of which solar systems evolved. Their ideas were wholly speculative. Herschel showed by direct observation that there are nebulae and was the first to develop the hypothesis by inductive methods. We know that the nebular hypothesis was received very calmly at first. There were a few outcries against it, but Laplace published it only as a speculative theory; an appendix to his "Celestial Mechanics," and Herschel did not publish his ideas except in scientific papers of the Royal Society, which were not read by the masses. The opposition from the theologians was not more vigorous than from most scientists. The exact nature of a nebula was not known. The question arose as to the difference between nebulae and star clusters. Was it simply a case of optical power of resolution? And again science halted for a time.

From an unexpected source came a new factor, the spectroscope which had no less an influence upon the time than had the telescope of Galileo. Fraunhofer, about the beginning of the nineteenth century, showed that there is an intrinsic difference between a gas and a solid. Immediately his principles were applied to the nebulae and they were found to be gaseous. "As a result," says MacPherson, "The chemical unity of the universe was found to be a truth of nature. The cosmos was seen to be in very truth a cosmos connected and interrelated in all its parts." Quoting further, he says:

> Slowly but surely the mechanical theory was passing away. Even in theology, Deism was giving place to the new sense. In philosophy, the empiricism of the French school was giving way before the idealism of Fichte, Schelling, Hegel and Goethe. And in physics, the whole mechanical concept of empty space and isolated bodies was disappearing.

As expressed by Charles N. Holmes, the universe began to expand before man:

> Above him yawn (ed) abysmal gulfs of space!
> Mysterious, majestic, silent, cold
> Ablaze with stars that shone upon our race
> When Joseph by his relatives was sold,
> Before him loom (ed) the sky like scroll unroll'd,
> Inscribed with symbols gleaming brilliantly,
> Far, far remote, yet those that furthest be
> Reveal no limits save Infinity.

A new problem now arose: What is the form and extent of our universe? If we accept the Copernican or heliocentric theory of our solar system, then what is our concept of the stellar system? The great galaxy or "Milky Way" suggests the existence of two streams of stars, or a spiral nebula, and the work of Kapteyn, Campbell, Shapley, Russell and Hubble, not to mention others, has gone far to establish it as a truth. The idea of "island galaxies" is again coming into favor. "This theory," says Eddington, "is much to be preferred as a working hypothesis, and its consequences are so helpful as to suggest a distinct probability of its truth." In the chemical and physical world we start with the electron as a unit from which we build the atom, then the molecule and finally the ordinary mass. In the cosmic universe the solar systems are the atoms with their planetary electrons. The star clouds and clusters are cosmic materials composed of solar systems, and the galaxies are aggregations of stellar and nebular masses. Says MacMillan:

> It will be observed that ordinary masses are just in the center of our list of physical units. Shall we go back to the old notion that we are the natural center of the universe, or shall we regard this as a mere appearance, due to the fact that it is more and more difficult for us to have experience with those units which are more and more remote from us in the physical scale? We are at the center, because the center is everywhere. Two atoms of gold seem just alike because we are not very familiar with atoms of gold, and two electrons seem to be identical merely because of our profound ignorance. Supergalaxies exist though we have had no experience with them at all; likewise, hypersupergalaxies, and so on indefinitely. Things do not cease to exist merely because we are ignorant. We should beware of the tacit postulate, which often crops out, "Only those things exist with which we have had experience." Nature is much broader than experience, and we must have plenty of room for expansion.

During the nineteenth century cosmogony has been in the hands of an able group of mathematicians, physicists, geologists and astronomers, among whom should be mentioned Moulton, MacMillan, Chamberlin, Kapteyn, Sears, Russell, Shapley, Jeans, Eddington, deSitter and Einstein. The old ring theory of Laplace has been rigorously attacked and found wanting in many of its details. We now have a new idea under the caption of the planetesimal hypothesis in which the modern concepts of dynamics and the structure of matter play an equally important rôle with mathematics. Geology is no longer ignored. The invention and application of the interferometer has verified and greatly extended the postulated sizes and distances of the stars. The new one hundred inch Hooker telescope is resolving and analyzing the star clusters and nebulae in a manner wholly undreamed of a half century ago.

The two solutions of Einstein's fundamental equations, resulting in a finite, static universe as one extreme and an infinite, expanding universe as the other, give promise of a more general solution approaching objective reality. We must not be misled by the first solutions of so difficult a problem. As often happens in pure mathematics, the special cases are the first to appear, then the more generic gradually evolve.

The size and shape of the universe is probably no more impossible of solution to-day than the size and shape of the solar system was in Ptolemy's day. To our finite minds a universe that requires a beam of light five hundred thousand million light years to circumnavigate it is infinite, but as Sir James Jeans says, "We are not terrified by the sizes of the structures which our own thoughts create, nor by those that others imagine and describe to us. The immensity of the universe becomes a matter of satisfaction rather than awe; we are citizens of no mean city. Again, we need not puzzle over the finiteness of space; we feel no curiosity as to what lies beyond the four walls which bound our vision in a dream."

Schiaparelli once called astronomy the science of infinity and eternity and the description is just. "These words," says MacPherson, "are often used by philosophers and theologians. Astronomy gives some definite sense of what they mean. The concepts of infinity and eternity are soul-staggering, but they are less difficult than those of limitation of space and time. To the higher thought, the chief contribution of modern astronomy is doubtless this sense of the infinity of space and the eternity of time."

THE PHYSIOLOGY OF CONSCIOUSNESS[1]

By Professor EDWIN G. BORING
HARVARD UNIVERSITY

My thesis this evening is that scientific psychology needs more than to become the physiological psychology that Wundt originally called it, and that we are not entirely without the means of proceeding in this direction. Psychology, it seems to me, needs to save for its own uses both consciousness and the nervous system, and it must have both if it is to survive.

Once upon a time psychology had some hope of getting along without a nervous system. There was a time when introspectionists, like Külpe and Titchener, would have hailed with avidity any step that brought psychology nearer to being a descriptive science of the facts of experience, a science that could get along with introspection as its only method and could leave the nervous system and the stimulus ruthlessly in the outer darkness of physiology. There is no need to explain to this audience that the introspective method unsupported failed to yield a psychology, or perhaps even a single factual generalization.[2] The most satisfactory introspective experiments were those that resulted in the correlation of sensory or perceptual data with stimulus. The best theories were formulated in terms of the nervous system or the sense-organs. Unaided introspection proved inadequate in crucial cases, as in the problem of thought where we were left with only a "physiological" determining tendency as a principle of explanation.

The reaction of behaviorism against this state of affairs by the complete rejection of the introspective method was very natural, even though it represented a throwing out of the baby with the bath. Theoretically you can answer for animals, by tests of discrimination or by observation of conditioned reflexes, any of the questions about sensory or perceptual

[1] Address of the retiring vice-president and chairman of Section I—Psychology, American Association for the Advancement of Science, New Orleans, December 29, 1931.

[2] There never were any laws of introspective psychology other than those that state the correlation of conscious processes with the stimulus or with events in the nervous system, with the possible exception only of the law of association. Now-a-days it is superfluous to claim that association is solely a law of conscious events, when we are so constantly being reminded of its physiological counterpart, the conditioned reflex.

capacities that have been answered for human beings by the use of the introspective method. And what can be done with animals in general can be done with human animals. Nevertheless, the behavioristic method is not always advantageous. Sometimes it yields results that are less univocal than those gotten by an introspective method.[3] Sometimes it is terribly laborious and no added precision is gained for the added pains. If you will imagine a behavioristic method which determines, without the use of words, the occurrence of difference tones, or the wave-length of one of the three pure spectral colors, you will see what I mean when I say that these objective methods can be clumsy.[4]

It is worth noting that behaviorism owes its *ism* to consciousness. And what would it be without its *ism*? Well, it would be physiology. Behaviorism has preserved itself as psychology and as something that is not physiology—I speak of the historical fact, whatever may have been the wishes of the behaviorists—by persistently attempting to solve the problems that originated as introspective problems of the psychology of consciousness. The attitude of the behaviorist for introspection has always been ambivalent. He has hated introspection for the love he bore it.

On the other hand, if we bring consciousness back into "physiological psychology," without ridding ourselves of the old-fashioned Cartesian dualism, we get no farther along than we have always been. Now there is nothing new in my objecting to dualism. Perhaps the majority of you had thought of dualism as already rooted out of psychology. Nevertheless, you see that the behaviorist, who would have us ignore consciousness in psychology, is thereby a dualist because he has to believe in consciousness as something different from the "objective" world in order to dismiss it as irrelevant. In this way behaviorism has often emphasized the fundamental dichotomy of mind and body by its insistence that the mind is not the body, and that you may take the one and leave the other. *Gestalt* psychology certainly has no use for dualism, and yet it is impossible to read Köhler[5] or Koffka[6] on the correspondence of "direct experience" to "underlying physiological processes" without feeling that the old dichotomy is still fundamental to their thought. The step I am asking you to take, in the interests of getting to a physiology of consciousness, is ever so much more radical than these imperfect attempts to avoid the Cartesian curse: I am asking you utterly to abandon dualism, sincerely, so that if there be a consciousness that could be ignored you will let it into the total system that is your scientific monism.

It is the introspectionists who have been primarily at fault in this matter. Wundt talked about "immediate experience," and Köhler talks about "direct experience." In such phrases there is an implication that there is some way of taking hold of experience, immediately, just as it is *per se*, and of keeping it for scientific purposes, and that in doing so one has introspection. Physical science is supposed to deal with entities that are mediate to experience, to be indirect in the sense that its subject-matter consists of inferential "constructs." The formula that Külpe and Titchener took from Avenarius, that psychology deals with all experience regarded as dependent upon the experiencing individual, is really not so much different, because experience really is dependent upon an experiencing individual for its existence, and when so regarded is thus being taken more immediately, more in its own right rather than as a ground for inference. I am quite serious when I say that this view of introspection seems to me to be nonsensical. The view implies that there is nothing important to introspection, that to have an experience is the same as to be aware of having it, that observation of conscious processes is nothing other than being conscious.[7]

However, the difficulty of regarding "direct experience" as the subject-matter of any science becomes more apparent when we note that we are landed by it in a circle of dependencies. At one time those who hold to this view will tell you that there is experience from which all science is derived, that psychology deals immediately with experience, and that the materials of physical science are mediately derived from experience. The view seems to make psychology prior to all the other sciences. Thus Köhler tries to prove that behaviorism is really introspectional because its data were originally experiential.[8] Nevertheless,

[3] On the behavioristic method as more equivocal than the introspective method in the determination of the two-point limen, see E. G. Boring, "The Stimulus-Error," *Amer. J. Psychol.*, vol. 32, pp. 449–471, 1921.

[4] *Cf.* the awkward (imaginary) experiment that J. B. Watson describes for the determination of difference tones, "Psychology from the Standpoint of a Behaviorist," Philadelphia, 1919, p. 78 (same in 2nd ed.). I have forgotten who it was in the Harvard Psychological Colloquium who suggested that an animal could be tested for *Hauptfarben* in the following manner. If one is after the wave-length for pure yellow, one would train the animal to respond positively to the yellower of two oranges, and would see how far along the spectrum this relational response would be given. Obviously, it should break down at the pure yellow, since a yellow-green is not yellower than a yellow.

[5] *Cf.* W. Köhler, "Gestalt Psychology," N. Y., 1929.

[6] *Cf.* K. Koffka, "Some Problems of Space Perception," in "Psychologies of 1930," pp. 161–187, Worcester, Mass., 1930.

[7] Philosophers have made this same objection, but introspectionists have found little force in it ever since the argument for immediacy was put so trenchantly by E. Mach, "Analyse der Empfindungen," 1886, *et seq.*, and Eng. trans.

[8] Köhler, *op. cit.*, pp. 3–69.

these introspectionists may at another time hold to the opposite point of view that experience is dependent upon the activity of a brain or a nervous system. "No psychosis without neurosis" used to be the phrase. What then have we?

We have first the assertion that the brain, a physical entity, is a "construct," like an atom or an electron, which is not as such given in experience but which may be regarded as real, and as generated from experience in the way that all scientific realities issue out of experience. In this sense the brain is dependent upon experience. However, in another sense experience must be considered as dependent upon the brain. To have each dependent upon the other is not a relationship that is going to help us much. Either dependency alone is valuable, but the two negate each other.[9]

On the other hand, we avoid this circle at once if we admit that psychology is not peculiar among the sciences, that introspection is as much a method as any of the other methods of observation, that it is a method whereby on the basis of experience we establish the existence or occurrence of mental "realities," like sensations or seen movements or any of the other phenomenal objects which introspection yields. The old-fashioned introspectionist will not like my calling a sensation a "mental object," but I mean that it is as much an object as ever a molecule is a physical "object." Certainly the sensation as such is not given in experience itself.

If any of you doubt this statement, you have only to think how science is always proceeding by indirection. It uses experience, yes; but always as symbolic of something else. The behaviorist misses this point when he tries to make behavioral observation more immediate than introspective. Watson said that introspection is verbal behavior, as if there were some virtue in preferring the immediate datum that the experimenter observes, the spoken words, to the conscious processes signified. The behaviorist who uses "objective" methods of recording is just as indirect as the introspectionist: the immediate datum may be a kymograph record, yet it is for him merely a symbol of behavior.

Now, at last, I come to the main issue of this paper, my thesis that *introspection is a method for the observation of certain events in the brain.* Traditional introspectionism would protest such a statement. If I see a red circle, it would say that I am not seeing the brain; no part of the brain is red, presumably no event in it is circular. Nevertheless, I may be observing the brain, just as I can observe animal behavior by looking at records from a kymograph, or as I can observe an electric current by looking at the black and white pattern which is a pointer on the scale of a galvanometer. In scientific observation we always come face to face with symbols, and usually we ignore the symbols and talk about the realities that they signify.

Such a symbolic function of introspection may, of course, be sound epistemologically and yet utterly wrong. A relationship of this kind either does, or does not, grow in the structure of a science. We can not force its acceptance by argument. All we can do is to find it implicit in thought and to bring it out into the open. Even when it is exhibited and accepted by every one, it remains as tentative and temporary as does all scientific truth. It is our task, therefore, to consider the extent to which this view has already found its way into psychological research and whether it has seemed to be thus far successful.

We can best understand what has been going on within psychology if we return to the dualistic tradition, and see that in it there were, roughly speaking, three principal loci for psychological events: (1) the sense-organs, (2) the central nervous system and (3) consciousness. The three are causally related: stimulation of the sense-organ gives rise to a central neural process, which in turn may be said to "cause" a conscious process. In the less exact parlance of the laboratory, we are always thinking about stimulus, brain and consciousness.

The events in the brain, the middle term of this dependent series, have been largely inaccessible to observation. There has been of course some extremely important "direct" experimentation, from Fritsch and Hitzig to Lashley. There has been clinical observation. The rest of what we "know" about the brain is at a higher inferential level. The physiologist holds to the faith that the brain, being made up of neurons, is capable only of that excitation which is the sum of the excitations of many neurons, and that these central neurons obey the same laws and are excited under the same limitations as apply to the peripheral neurons which have been experimentally studied. To this article of faith the psychologist sometimes opposes another belief, that the organization of cerebral excitation corresponds to the organization of phenomenal experience. These two hypotheses are not necessarily consistent, and often we have to choose between them.

The two end-terms of the dependent series, the stimulus and consciousness, have been much more accessible to observation. The result is that the largest body of precise information within experimental psychology consists of correlations between these two terms. These correlations are the facts which make up the

[9] This difficulty is considered at length in Boring, "The Psychologist's Circle," *Psychol. Rev.*, vol. 38, pp. 177–182, 1931.

chapters on sensation and perception in any psychological handbook.

If the scientific mind could be satisfied with correlations, physiological psychology might have ignored the brain as inaccessible to its methods, and have remained content with correlations between stimulus and sensory process in the old days and between stimulus and response later on. The experimental method itself yields, in the first instance, mere correlations; nevertheless, the scientist demands something more. He wants insight into the relationships, a complete and immediate understanding which seems to leave no further questions to be asked. Many persons, in learning to extract square roots by the use of a calculating machine, discover for the first time the rule that the sum of a given number of consecutive odd numbers equals the square of the number of numbers summed; the sum of the first *three* odd numbers, 1, 3, 5, is 9, the square of *3*. Such a relation is a mere correlation and it seems, when first discovered, a great mystery. Even the proving of the general rule by algebra may seem to leave the mystery intact. If, however, one draws big geometrical squares made up of unit squares, one sees at once why to any square one must add a "next odd number" of squares in order to get the next larger square. The mystery has gone and we have what I am calling insight.

It is this need for insight that has forced the brain upon psychologists. The gross psychophysical correlation must be made more intimate. We want, in Fechner's phrase, an "inner psychophysics." Nevertheless, even if we had this knowledge of the brain and the resultant correlations, we should still be wanting insight in both parts of the picture. We should, on the one hand, want more intimate knowledge of the relation of stimulus to brain, and this sort of knowledge we are now actually beginning to get in the all-or-none law of neural excitation, in the frequency theory of intensity that Adrian has so ably promoted, and in the experiment of Wever and Bray on the nature of the impulses in the auditory nerve. Very slowly this physiological continuity is getting worked out. On the other hand, the correlation between consciousness and the events in the brain shows no signs of yielding to insight because there is no conceivable way in which insight can transcend the dualistic gap between mind and body. If there were any ground for dualism, if immediate experience as such seemed capable of scientific study, we might shrug our shoulders and decide to make the best of an unsatisfactory situation. Since, however, dualism seems both to fail to give us a satisfactory scientific dichotomy and also to exclude insight from psychophysiology, we ought, it seems to me, to make all haste to abandon it.

Let us now get down to business and see what there is to be said, for the purposes of psychology, about events in the brain. We had best accept, I think, four of Titchener's dimensions of consciousness[10] as setting the main topics for investigation. Consciousness is organized in respect of four dimensions: quality, intensity, extensity, and the temporal dimension which Titchener called "protensity." We may begin with intensity.

Twenty years ago the physiology of intensity offered little difficulty. A strong stimulus gives rise to an intense sensation; presumably the middle of this causal sequence must consist of strong excitation. However, this simple view became untenable with the acceptance of the all-or-none theory of excitation of the neuron. It seemed at first as if a multiple-fiber theory of intensity were the only remaining possibility, that degrees of sensory intensity must depend upon the number of fibers stimulated. Then came the frequency theory, the generalization, now well established, that a stronger stimulus may excite a greater frequency of impulses in a single fiber.[11] It is not necessarily true that the frequency theory of intensity must displace the multiple-fiber theory. Some of Hecht's conclusions point to the possibility that the stronger stimulus gives rise both to the excitation of a greater number of fibers and to a greater frequency of excitation in the fibers stimulated.[12] The volley theory of Wever and Bray is also such a view.[13]

When such theories are being discussed, it is natural to ask whether there must not be a summation of separate impulses in the brain. I believe that such a question generally indicates the existence of an implicit belief that introspection gives direct information about the brain, that, since sensory intensity is not anything like a frequency or a spatial dispersion, the brain ought somehow or other to collect the separate impulses in order to get some single unitary state that corresponds to what phenomenal intensity itself seems to be. The multiple fiber theory calls for the summation of the impulses in separate fibers; the frequency theory calls for the summation of successive impulses in the same fiber; and any combination of the two theories calls even more for summation, since, when different causes give the same effect, we want some insight into how the different causes are effectively the same.

[10] E. B. Titchener never explicated his doctrine of conscious dimensions beyond his twelve-line note in *Amer. J. Psychol.*, vol. 35, p. 156, 1924.

[11] *Cf.* E. D. Adrian, "The Basis of Sensation," New York, 1928.

[12] See the summary of S. Hecht's views, H. Hoagland, "The Weber-Fechner Law and the All-or-none Theory," *J. General Psychol.*, vol. 3, pp. 354–359, 1930.

[13] E. G. Wever and C. W. Bray, "Present Possibilities for Auditory Theory," *Psychol. Rev.*, vol. 37, pp. 365–380, esp. 376–380, 1930.

Now I think we can indicate positively the sense in which summation must occur on any theory of intensity, but before I go into that matter I wish to point out how already prevalent is this view of the direct correspondence between consciousness and the brain.

Most psychologists have accepted this conception as a matter of course. It is the common assumption in different physiological theories of Weber's law, the theories of Wundt and G. E. Müller. Köhler is the most courageous modern to state the general view. He thinks of intensity as the electrical charge of a concentration of ions in the nervous tissues. He dislikes a constancy hypothesis between stimulation and central excitation, and so he suggests how the logarithmic relation of Weber's law might occur on the purely physiological level. However, Köhler likes a "constancy hypothesis" of the relation of consciousness to the brain (of course, he does not call his hypothesis by that term), so that there he supposes a simple relation between gradients of intensity and of electrical potential.[14] This notion of Köhler's, that there is a direct and simple correspondence between consciousness and events in the brain, only just misses what I take to be the necessary denial of dualism.

The physiologists are apt to avoid the problems of consciousness, but when they consider them they tend to make the same assumptions as the psychologists. Let me illustrate by reference to a recent article of Hoagland's on the Weber-Fechner law. This law requires that the plot of the measure of sensation against the logarithm of the stimulus should be linear. Hoagland, citing Hecht, points out that the Weber-Fechner law is known not to hold at the extremes, and that this semi-log plot may be, not linear, but sigmoid in shape, thus corresponding to other functions familiar to physiologists.[15] He cites certain cases where what we may call "excitation" does show this kind of dependence upon the logarithm of the stimulus. He cites Hecht's analysis of König's data for the visual discrimination of brightnesses as proving the same point. What can one conclude? That Hecht and Hoagland, at least, have rejected dualism and are ready to accept introspection as a measure of physiological excitation, for König's data were introspective, and they are ready to bring them under a physiological generalization.

Certainly then it is good form to assume that sensory intensity is a symbol of neural excitatory intensity. Can we justify the view further?

Here it is, I think, that we need to appeal to the physiology of introspection. Let us suppose that the occurrence of a given sensory intensity leads to a judgment of its degree. It does not matter whether that judgment is a word, spoken aloud or written on paper, in English or German, or whether it is the pressing of a key connected with an apparatus, or whether it is some imaginal form of note-taking which only later leads to expression. The judgment differentiates *the* intensity from other intensities, and as such it is a response to *the* intensity. In the absence of such response there can be no knowledge that the intensity occurred and hence no introspection.

However, the discriminative response is a response *to* the intensity, which one must now think of in physiological terms. Do the excitations of many fibers have to be summated in order that a response may occur to them? Do the successive excitations of a single fiber have to be summated to lead to a response that depends upon the frequency? Not necessarily in any objective sense, if summation implies that all the impulses are collected together into one place at one instant. On the other hand, there is summation in the sense that the response is to the totality of the impulses, that all the impulses are collectively effective in producing a single response which is characteristic of them all as a totality. For instance, if the neural impulse is electrical in nature, there need never be a summation of ion charges, but there must be a functional summation into a single physiological effect.

The experiment of Wever and Bray illustrates this point beautifully.[16] They hooked an electrode on the central end of the cat's auditory nerve where it enters the medulla; they talked to the cat and greatly amplified the currents of action in the nerve, leading the amplified currents to a loud speaker. Human speech was heard over the cat's nerve with little distortion and tonal frequencies up to 4000 cycles were accurately transmitted. It is still extremely doubtful whether a single auditory fiber can transmit a frequency greater than 1000 cycles. However, Wever and Bray transcend this difficulty in their "volley theory" which combines the multiple-fiber and the frequency theories of intensity. I can not expound this theory here, but the point of it is that, if a tonal frequency and amplitude, corresponding respectively to a given pitch and intensity, are put into the ear, the resultant excitation in the auditory nerve is utterly different, consisting of a number of frequencies in a number of fibers, and yet a simple electrode is able, out of the total effect of the excitations within the nerve, to pick up the original frequency and amplitude. The electrical circuit "responds" to the

[14] *Cf.* Köhler, "Die physischen Gestalten in Ruhe und im stationären Zustand," Braunschweig, Germany, pp. 211–227, 1920.

[15] Hoagland, *op. cit.*, 351–370.

[16] Wever and Bray, "The Nature of Acoustic Response: the Relation between Sound Frequency and Frequency of Impulses in the Auditory Nerve," *J. Exper. Psychol.*, vol. 13, 373–387, 1930.

totality of events in the nerve, without the original frequency and amplitude ever being reinstated in the nervous tissues. In the same way the physiology of sensory intensity must be at least a simple totality of degree of excitation, even though it may not be summated at any point at any one instant of time.

If we turn from the problem of intensity to the problem of extensity we find that ever so much more has been written about this problem and that the solution is less certain.

The oldest theory of extensity is the projection theory. It is a theory that was helped a hundred years ago by the theory of the specific energy of nerves, for, if the mind perceives, not objects, but "the states of the nerves" (as Johannes Müller said), it can "perceive" spatial pattern only if it be projected upon the brain. Now-a-days, Köhler, while denying projection, nevertheless keeps our thought in the same channels by his principle of correspondence between the spatial order of phenomenal experience and the underlying physiological processes.[17] It is certainly good form to suppose that the perception of shape and size is dependent upon spatial differentiation in the fields of central excitation.

There is not the same amount of supporting evidence for this view as there was in the parallel case of intensity. Nevertheless, I think that the conclusion for intensity helps us to a belief in a broad conception of correspondence for all the dimensions of consciousness. It looks as if some sort of physical intensity (like electrical potential) were the physiological fact of sensory intensity. It looks as if spatial differences of stimulation were the occasion of spatial differences of central excitation, and temporal differences of stimulation, of temporal differences in central excitation. We are not in such a statement making an appeal to analogy; we are saying that the physical dimensions of peripheral stimulation are likely to be the dimensions of the organization of central events, that there are not enough possible dimensions for us to expect a change of kind, and that the adequacy of perception to certain dimensions of the external world is thus readily explained.

The projection theory of extensity would thus be a very acceptable theory were it not inadequate to the facts. Since we can not undertake to review the entire field of space perception, let us select for consideration two special problems: the problem of the third dimension in vision and the problem of visual size.

Koffka has recently suggested that the existence of the third dimension in visual perception implies a tridimensional neural pattern in the brain.[18] Such a view is consistent with Köhler's theory of correspondence, but I think that a stronger case can be made out for it than a mere appeal to an uncertain generalization. So much has been said about the dependence of the perception of depth and distance on convergence, accommodation and retinal disparity, that Koffka is at pains to take the cases of depth that occur in simple drawings, where neither accommodation nor any binocular *differentia* is possible. He shows that you may see the Necker cube in either perspective, but that you practically can not see it as a flat geometrical design in the plane of the paper. What is the difference then between the plane design and the two perspectives? As projections they would be identical. If the two-dimensional pattern means a two-dimensional field of excitation, it is almost inevitable to look for a tridimensional field when the third dimension comes in immediately to the perception.

The case becomes stronger when we consider how retinal disparity works in stereoscopic vision. Let us think of the case of the truncated cone, which stretches out convexly toward the observer in stereoscopic vision. Each eye sees only a small circle within a large circle, but the relation of the small circle to the large circle is disparate for the two eyes. When the eyes first view the two drawings, there may appear two completely separate images in perception. Then the eyes move in respect of each other until there is seen, let us say, but a single large circle. At this stage the small circles may remain double within the single large circle. Thus far the experience fits the projection theory. The two large circles appear as two circles until the eyes move so that they lie upon corresponding points; then there is but one circle, because the two circles are projected upon the same locus in the brain. Moreover, we see that the eyes tend to move so as to bring similar images upon corresponding points. It is as if the mechanism of vision operated in the interests of simplification.

However, the eyes can never move so as to make both the large and the small circles coincide at the same time. If the small circles coincide, the large circles must be double, and *vice versa*, if we keep to projective geometry. However, projective geometry is just what perception does not preserve. Presently both small and large circles coincide and we see the truncated cone as a solid. Is not the conclusion almost inescapable that the tendencies for the large circles to combine and the small circles to combine are realized by the establishment of the circles in different fields, which for bidimensional figures would have to be separated in a third dimension?[19]

[17] Köhler, "Gestalt Psychology," *op. cit.*, p. 64.

[18] Koffka, *loc. cit.*

[19] There is a very simple system of geometrical projection in which a disparity, which is like retinal disparity, actually gives the projection in the third dimension, but I forego its discussion since I can not make it seem like acceptable physiology.

Now let us turn to the problem of visual size. It seems probable that the perceived size of stimuli at the same distance from the observer is proportional to the size of the corresponding retinal images. As usual we can begin with a projection theory.

However, a projection theory breaks down when we consider size in relation to distance. As a stimulus-object is moved away from the observer its perceptual size decreases, but it decreases not nearly so fast as does the size of the retinal image. The alley experiments have worked out the law of the dependence of phenomenal size upon distance. The facts are as if perception compromised between the projection theory and some other theory, under which a given object would maintain its size, irrespective of distance.

There is another way in which size varies. The moon in the zenith is perceptually smaller than the moon on the horizon. For all the controversy that has gone on about this illusion, it seems fairly accurate to say that the size of the moon or of any other stimulus-object is diminished when the head and the eyes assume the strained position required for looking at the zenith and *when distance is indeterminate*. It is this second condition that has fooled the experimenters. The illusion fails, or is reduced to a few per cent., for an artificial moon a few feet from the observer. Schur showed that the illusion may be as much as 50 per cent. for cardboard moons 33 meters from the observer. Beyond 33 meters in the vertical it is not possible to carry most experiments, but the implication of Schur's results is that position of the head and eyes makes a difference to size when distance is indeterminate, as it becomes when it is great. With shorter and determinate distances, the laws of the alley experiments hold. With the moon, distance is completely indeterminate and the illusion is maximal.

It thus appears that perceptual size is a complex function of retinal size, of distance, and at times even of the position of the head and eyes. Moreover, there is some ground for belief that this variation in size applies to three-dimensional fields. It is very hard to adjust this sort of physiology to the conventional notions of neuron reflex arcs. These phenomena accord much better with Lashley's principles of equipotentiality and mass action in the cerebral cortex.[20] Hunter has criticized Lashley's views on the ground that it is possible to explain the animal behavior in question in terms of a more conventional physiology.[21] The difficulty with Hunter's position, so it seems to me, is that conventional physiology, even if it can explain Lashley's data, can do very little else for the theory of perception. What, for instance, can it do for these problems of the visual perception of solidity and of size?

I am not proposing that we disregard facts in favor of theories. I am proposing merely that we accept, tentatively, the most productive hypotheses, those with the greatest resolving power. We may need some day to abandon the hypothesis that consciousness always involves some kind of brain action. However, I think that this view should be kept just now, and with it the more explicit view that spatial and intensive phenomena, given in introspection and representing respectively spatial and intensive aspects of the stimulation, are symbols of spatially and intensively differentiated events within the brain. The evidence for such an hypothesis is scanty enough, goodness knows, but I think it is greater than the grounds for faith in the simple reflex-arc theory of the brain. If we must choose an hypothesis, let us choose one that gives us some insight into the extensive knowledge of perception which experimental psychology includes to-day.

There is no time for me to discuss the other two dimensions of consciousness. Of protensity, the temporal dimension, we know but little. We should be looking, with Köhler, for durations in the brain when introspection shows duration to be a characteristic of the perception.

Quality has had no acceptable physiological hypothesis for itself since the theory of the specific energy of nerves and the related theory of sensory centers broke down. All we can be sure of is this: whatever quality is within the brain, it must be differentially dependent upon whatever quality is within the stimulus. In tonal hearing this view means a frequency theory of quality, and Wever and Bray have shown how such a view is not necessarily incompatible with the theory of peripheral frequency for intensity. In the other senses we are dependent upon more knowledge of the receptor processes.[22]

Let me see if I can now, in closing, repeat all that I have said in the compass of a few words.

Dualism is dead. It ought to be buried. It can not work for us any more and we do not need it.

The great delusion of psychology has been the be-

[20] The standard reference is K. Lashley, "Brain Mechanisms and Intelligence," Chicago, 1929; but perhaps the clearest exposition of this point is his "Mass Action in Cerebral Function," SCIENCE, vol. 73, pp. 245–254, 1931.

[21] *Cf.* W. S. Hunter, "A Consideration of Lashley's Theory of Equipotentiality of Cerebral Action," *J. General Psychol.*, vol. 3, pp. 455–467, 1930.

[22] Perhaps this view accords with J. P. Nafe's objections to the theory of the specific energy of nerves and thus with his quantity theory of feeling; *cf.* his discussion in "The Foundations of Experimental Psychology," pp. 395–399, Worcester, Mass., 1929. I have never understood Nafe's theory; but if he means that quality must ultimately be understood as a function of the quantifiable aspects of nerve-conduction and the relationships that are quantitatively statable between excitations, then I suppose that all scientifically minded psychologists would immediately agree with him.

lief that we can have a science of direct experience. Scientific facts come out of experience, but they are then no longer in it. Science does not attempt to reconstitute experience; it builds up inferentially a world of constructs which are its realities.

A careful examination of the introspective process shows that introspection, like any other observation, is the taking note of symbols that mean occurrences in this constructural or real world.

We are, therefore, free to examine these symbols, the phenomenal data of introspection, to see what they can symbolize with the greatest profit for scientific psychology; and we conclude that neural events are the sort of mental constructs that introspective data most effectively "intend."

We can then set out to test this view, to see what it will yield us in the way of a physiological psychology. Such a view is necessarily subject to test and to correction, in the same way that a galvanometer is subject to test and correction as to how it means or "intends" the strength of an electric current.

When we go to the physiological theories of psychologists (and of some physiologists, too) we find many views consonant with the thought of the present paper, as indeed we could have known from the start, since the paper has been written to explicate and evaluate these views.

In general, the most plausible theory of the brain seems to be that the four conscious dimensions find reality there in four physical dimensions of intensity, extensity, duration, and an uncertain fourth which must have an immediate dependence upon the physical variable for quality in the stimulus.

Such a general view is most definitely explicable for intensity. Sensed intensity must represent degree of excitation in the brain. Such excitation does not, however, have to be localized at a single place at a single time, except that it must all be effective in producing a simple subsequent neural event, which is the first physiological term of the introspective process.

In respect of extensity, the notion that introspection tends approximately to mirror the brain is, at the present day, a plausible view and a useful one. A more conservative physiology not only leaves one without an hypothesis for most of the facts of space perception, but implies certain limitations which are contradicted by the facts.

Finally, in urging this view upon you for serious consideration, I would make bold to remind you that scientific hypotheses and scientific truth are temporary and provisional, and that hypotheses that are false to-day have been largely instrumental in leading us to what is true to-day. However, I doubt if a false hypothesis ever led far toward the truth unless it was at the time believed to be true. You have in that statement both my admonition and my apology.

SCIENTIFIC EVENTS

THE MASTER'S DEGREE FOR POSTGRADUATE STUDENTS IN THE MEDICAL SCHOOL OF COLUMBIA UNIVERSITY

ACTION to establish higher standards in the practice of surgery and other specialties of medicine has been taken by the Columbia University Council. Dean Willard C. Rappleye, of the Medical School, characterized the step as having "important significance of a public character." He said:

> The university has adopted a standard of training in each of the clinical specialties, successful completion of which will carry the degree of master of science. The university, however, will not grant recognition for postgraduate training for less than that which will qualify the man as competent in the specialty concerned.
>
> The time will come in this country, as it has in others, when the public and the profession will demand that only those who are properly trained to do major surgery, for example, will be permitted to do it. At the present time large numbers of doctors are doing surgery who are quite incompetent and untrained.

The new regulations for the degree of master of science in postgraduate medical education follow:

The university grants recognition for acceptable postgraduate work in the clinical specialties by means of the degree of master of science.

This degree is non-specific, that is, it does not carry a designation of the special field of study to which the student has devoted himself. Only a broad definition of requirements is stated in order to permit flexibility in the training for the various clinical specialties and adaptation of that training to the needs and preparation of each student. The specific requirements for each of the specialties are formulated by the departments concerned.

A student who wishes to secure the degree of master of science in postgraduate medical work must present evidence of graduation from a medical school approved by Columbia University, and completion of an internship of not less than one year after graduation in a hospital approved by Columbia University.

Students who offer work pursued in other universities, laboratories or hospitals for part fulfillment of the requirements for the degree of master of science as hereafter set forth, should file a certified statement of such training with the director of university admissions for evaluation.

A student admitted to the university for postgraduate medical studies who wishes to become a candidate for the

degree of master of science will be registered upon recommendation of the department in which his work is to be done.

The requirements for the degree of master of science are:

A period of study after the internship of not less than three years in the university or in hospitals and laboratories recognized by it, at least one calendar year of which must be spent in this university.

Such intensive graduate training in the basic medical sciences of anatomy, embryology, physiology, biochemistry, pharmacology, pathology, bacteriology, and in other fields of science as shall be recommended by the departments concerned and approved by the Administrative Board on Postgraduate Studies in Medicine.

An active experience during the three-year period of not less than eighteen months in the hospital, clinics and diagnostic laboratories of the specialty elected.

Written, oral and practical examinations and a dissertation may be prescribed in the specialty elected and in clinical, laboratory and public health fields to which the specialty is related.

A student admitted to the university for postgraduate medical training may become a candidate for the degree of doctor of philosophy if he meets the requirements for that degree that are prescribed by the faculties of political science, philosophy and pure science.

FURTHER INVESTIGATION OF COSMIC RAYS

PLANS for a concerted effort to discover the source and nature of cosmic rays, involving the measurement of these radiations at eighteen widely scattered sites on the earth's surface, are disclosed by Dr. Arthur H. Compton, professor of physics at the University of Chicago.

During the spring and summer of 1932 more than a dozen physicists, working in several parties under the direction of Dr. Compton, will test the intensity of the rays at thirteen sites. Electrometer readings, taken largely in mountain ranges, will be made in Panama, Peru, New Zealand, Australia, Hawaii, Alaska, the Argentine, Chile, Kashmir, Ceylon, Singapore, Java and South Africa. The Carnegie Foundation and the University of Chicago will share the expense of the study.

The present expeditions continue the work which Professor Compton and his collaborators carried on last September and October on Mount Evans, Colorado, and on the Jungfrau in the Swiss Alps. The projected measurements will be made at widely distributed stations and at different altitudes on mountains ranging in height from 7,000 feet to 26,000 feet.

The objective of the expeditions, according to Dr. Compton, is "more complete knowledge of the nature and place of origin of the cosmic rays. A survey such as this should give the most adequate test that has yet been devised to distinguish whether the cosmic rays are photons, such as light and x-rays are, or electrons, such as give rays to the earth's aurora. Because of the effect of the earth's magnetic field, electrons should give less intense rays near the equator than near the poles. Likewise, if the cosmic rays have their origin in the earth's atmosphere there should presumably be variations with the geographical location."

Dr. Compton, who for several years has been measuring the rays in Eckhart Hall on the campus of the University of Chicago, will himself do a considerable share of the proposed work. Accompanied by his wife and his 14-year old son, Arthur Alan, who assisted him in the work on Mt. Evans, Dr. Compton will leave for Panama in March, to make tests on Mt. Chico, fifty miles from the Canal. His next stop will be Peru, where he will work in cooperation with the Carnegie station at Huancayo, making measurements over as wide a range of altitudes as possible and especially at very high altitudes.

Mt. Cook in New Zealand will be the next objective of the party, and the fourth point of observation will be Mt. Kosciusko between Sydney and Melbourne in Australia. From there Dr. Compton will proceed to Hawaii to set up his apparatus on Mauna Kea in Hawaii, and will then go on to Alaska, where Mt. McKinley has been chosen as the experimental site. Late in the summer Dr. Compton will return to America to join Dr. J. C. Stearns, of Denver University, and Dr. R. D. Bennett, of the Massachusetts Institute of Technology, both of whom have cooperated with him in previous cosmic ray studies, and will have spent the summer of 1932 in making further measurements in Colorado.

Three other cooperating parties will report to Dr. Compton at the end of the summer. One will take measurements on the Volcano Lanin in Patagonia and at Punta Arenas in Chile. A second, in charge of Professor S. N. Naude, recently a research fellow at the University of Chicago and now on the faculty of the University of Cape Town, will climb Mt. Winterhoek in South Africa, and will probably measure also the cosmic rays at Mt. Brukkaros.

In India, Professor J. N. Benade, of Punjab University, Lahore, will go to Mt. Nunga Purbat in Kashmir, the third highest peak in the world, and will, if possible, make tests at several altitudes ranging as high as 20,000 feet or more. Professor Benade will then proceed to Kandy, Ceylon, Singapore, Straits Settlement and Mt. Tjerimai, Java, for further measurements.

Negotiations are also being made with several independent groups, which are planning expeditions to

the north and south polar regions within the next two years for the inclusion of physicists in the parties, to make still further cosmic ray measurements.

PRESENTATION OF THE PERKIN MEDAL TO DR. BURGESS

THE Perkin Medal, bestowed annually on "the American chemist who has most distinguished himself by his services to applied chemistry," has been awarded for 1932 to Dr. Charles Frederick Burgess, president of C. F. Burgess Laboratories, Inc., for "a lifetime of accomplishment" in this field.

The medal will be presented at a joint meeting of the Society of Chemical Industry, the American Chemical Society, the Electrochemical Society, and the Société de Chimie Industrielle at 8:30 P. M. on January 8, at the Hotel New Yorker.

Dr. Burgess will speak on "Research 'for Pleasure or for Gold.'" Howard F. Weiss, of New York, will describe the achievement of the medalist, and Professor Marston T. Bogert, of Columbia University, will present the medal. Dr. Allen Rogers, of Pratt Institute, Brooklyn, chairman of the American Section of the Society of Chemical Industry, which awards the medal, will preside.

The work of Dr. Burgess in applied chemistry and electrochemistry, done chiefly at Madison, Wisconsin, embraces for the most part the fields of electrolysis, electrolytic iron and its alloys, the metallurgy of zinc, the corrosion of iron and other metals, and the development of the dry cell.

He devised a method and apparatus for sterilizing liquids with nascent chlorine which has been used in Madison hospitals to treat badly infected wounds and gangrene, and by explorers for sterilizing drinking water. This method bears some relation to the successful chlorine sterilization treatments later used during the war.

Dr. Burgess was born June 5, 1873, in Oshkosh, Wisconsin. In 1895 he was graduated in electrical engineering from the University of Wisconsin, where he served as instructor and assistant professor for five years following his graduation. There he established a course in applied electrochemistry, the first in the United States and later he established the chemical engineering course. In this academic atmosphere he demonstrated that scientific research was of the highest value to industry. In 1910 he established the C. F. Burgess Laboratories to demonstrate the marketability of chemical research.

Dr. Burgess's early work in applied electrochemistry became of industrial importance. He devised a simple electrochemical method for removing the surplus brazing metal from the brazed iron bicycle frame. He demonstrated the commercial utility of the electrolytic cleaner, universally adopted for cleaning metals preparatory to electroplating, and perfected the fused salt aluminum electrolytic rectifier, since manufactured extensively.

In 1904 Burgess and Hambuechen presented their paper on "Electrolytic Iron" which has been the basis for the commercial production of electrolytic iron both in the United States and France. Shortly afterward Dr. Burgess was given a grant of $10,000 by the Carnegie Institution to continue this work. This grant resulted in an extensive research on electrolytic iron and its alloys, several thousand of which were made and investigated.

The work of Dr. Burgess in corrosion has been of importance in its commercial aspects. He applied the principle of over-voltage to dry cell construction when the price of zinc mounted rapidly during the war. He substituted terne and tin plate for the zinc bottoms in dry cells at a considerable saving in cost.

He did a large amount of work on stray current electrolysis and made surveys in many cities in the United States. Dr. Burgess has done a considerable amount of work in improving hot galvanizing and electrogalvanizing. He was granted a patent in 1908 for separating articles in the electric furnace to prevent their fritting together in the intense heat of the reaction zone. This method is now used extensively in electric furnace practice.

Other achievements of Dr. Burgess are the electrochemical production of white lead and chrome yellow, electroplating on aluminum, and a method for soldering aluminum. He devised methods of roasting zinc ores and then concentrating magnetically. He succeeded in having gas put on a heating value basis in Wisconsin, the first state to adopt this standard. During the war, he was instrumental in devising on a large scale successful methods for producing and purifying silicon and titanium tetrachlorides. In the dry battery field he has made many contributions.

Dr. Burgess is also president of the Burgess Battery Company, the Burgess Building Company, the Burgess-Parr Company and the Burgess Dry Cells, Limited, of Winnipeg. The five companies of which he is the head are the outgrowth of his effort to carry chemical engineering research to industry. They employ more than 1,000 workers and turn out about $6,000,000 of products annually.

The Perkin Medal was founded in 1906 at the time of the Perkin semi-centennial celebration of the coal-tar discoveries, the first medal being awarded to Sir William H. Perkin himself.

OFFICERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

A FULL account of the New Orleans meeting of the American Association for the Advancement of Sci-

ence and the scientific societies associated with it, edited by the permanent secretary, will be published in the issue of SCIENCE for February 5. Officers of the association were elected on December 31 as follows:

PRESIDENT

Dr. John J. Abel, professor of pharmacology, the Johns Hopkins University.

VICE-PRESIDENTS

A—*Mathematics.* Professor Howard H. Mitchell, University of Pennsylvania.

B—*Physics.* Professor David L. Webster, Stanford University.

C—*Chemistry.* Professor Frank C. Whitmore, Pennsylvania State College.

D—*Astronomy.* Dr. Paul W. Merrill, Mount Wilson Observatory.

E—*Geology and Geography.* Professor Wm. H. Hobbs, University of Michigan.

F—*Zoological Sciences.* Professor Charles Zeleny, University of Illinois.

G—*Botanical Sciences.* Dr. H. L. Shantz, University of Arizona.

H—*Anthropology and Archeology.* Professor C. H. Danforth, Stanford University.

I—*Psychology.* Dr. Walter S. Hunter, Clark University.

K—*Social and Economic Sciences.* Professor William F. Ogburn, University of Chicago.

L—*Historical and Philological Sciences.* Dr. Waldo G. Leland, permanent secretary and executive director of the American Council of Learned Societies.

M—*Engineering.* Professor Dugald C. Jackson, Massachusetts Institute of Technology.

N—*Medical Sciences.* Professor Wm. H. Park, University and Bellevue Hospital College Bureau of Laboratories; director of the New York City Health Department.

O—*Agriculture.* Professor J. H. Gourley, Ohio Experiment Station, Wooster.

Q—*Education.* Dr. S. A. Courtis, University of Michigan.

ELECTED MEMBERS OF THE COUNCIL

Professor Arthur H. Compton, University of Chicago.
Mr. Austin H. Clark, Smithsonian Institution.

EXECUTIVE COMMITTEE MEMBERS

Professor D. R. Curtiss, Northwestern University.
Professor Joel H. Hildebrand, University of California.

GRANTS COMMITTEE

Dr. Philip Fox, director of the Adler Planetarium and Astronomical Museum, Chicago.
Dr. William Crocker, director of the Boyce Thompson Institute for Plant Research, Yonkers, New York.

MEMBER OF THE FINANCE COMMITTEE

Dr. Arthur L. Day, director of the Geophysical Laboratory, Carnegie Institution, Washington.

REPRESENTATIVE ON BOARD OF TRUSTEES SCIENCE SERVICE

Professor Raymond Pearl, School of Hygiene and Public Health, the Johns Hopkins University.

SECRETARY OF SECTION C—CHEMISTRY

Dr. Reynold C. Fuson, University of Illinois.

SECRETARY OF SECTION H—ANTHROPOLOGY

Professor Carl E. Guthe, director of the Museum of Anthropology, University of Michigan.

SCIENTIFIC NOTES AND NEWS

THE thousand dollar prize given each year by the American Association for the Advancement of Science "for a notable contribution to science" was awarded at New Orleans to Dr. Carl Caskey Speidel, of the University of Virginia Medical School, for his paper concerned with the growth of nerve cells.

THE Astronomical Society of the Pacific has awarded its Catherine Wolfe Bruce Gold Medal "for distinguished services to astronomy" for the year 1932 to Dr. J. S. Plaskett, director of the Dominion Astrophysical Observatory, Victoria, British Columbia, who was recently elected Savilian professor of astronomy at the University of Oxford to succeed the late Professor H. H. Turner.

FRIENDS of Professor Chandler presented to the Trustees of Columbia University in 1910 a sum of money which constitutes the Charles Frederick Chandler Foundation. The income is used to provide a lecture by an eminent chemist and a medal for him. The lecturer this winter will be Professor James Bryant Conant, chairman of the division of chemistry, Harvard University. His research has included work on free radicals, hemoglobin, reduction and oxidation of organic compounds and quantitative studies of organic reactions. The lecture, entitled "Equilibria and Rates of Some Organic Reactions," will be given in Havemeyer Hall, Columbia University, on February 5, at 8:15 P. M.

DR. LOUIS B. WILSON, professor of pathology at the University of Minnesota and director of the Mayo Institute, was elected president of the Society of Sigma Xi at the New Orleans meeting, succeeding Dr. G. W. Stewart, head of the department of physics at the State University of Iowa.

AT the Andover meeting of the American Anthropological Association Dr. John R. Swanton, of the

Bureau of American Ethnology, was elected president for the meeting to be held next year at Atlantic City.

PROFESSOR REGINALD A. DALY, of Harvard University, was elected president of the Geological Society of America at the recent meeting at Tulsa, Oklahoma. He succeeds Professor Alfred C. Lane, of Tufts College. The following vice-presidents were chosen: Professor Nevin M. Fenneman, University of Cincinnati; Dr. W. E. Wrather, of Dallas, Texas; Dr. R. S. Bassler, U. S. National Museum, and Professor A. N. Winchell, University of Wisconsin. Other officers are: *Secretary,* Dr. Charles P. Berkey, Columbia University; *Treasurer,* Dr. Edward B. Mathews, the Johns Hopkins University, and *Editor,* Mr. Joseph Stanley-Brown, of New York City. New councilors are Dr. George W. Stose, U. S. Geological Survey, and Dr. Frank R. Van Horn, Case School of Applied Science.

DR. L. V. REDMAN, who has served as president-elect of the American Chemical Society during 1931, becomes president with the new year. By vote of the council the following have been elected to the offices indicated for the terms beginning January 1, 1932: *President-elect,* Professor A. B. Lamb, of Harvard University, editor of the *Journal of the American Chemical Society;* director for the second district, comprising the States of New York and New Jersey, E. M. Billings, Eastman Kodak Company, Rochester, New York; director for the fifth district, comprising the States of Illinois, Indiana, Michigan and Wisconsin, Professor Roger Adams, University of Illinois; director-at-large, R. E. Wilson, director in charge of the Development and Patent Department, Standard Oil Company of Indiana; councilors-at-large, Dr. G. J. Esselen, Jr., Boston; Professor Ross Aiken Gortner, University of Minnesota; Dr. E. Emmet Reid, the Johns Hopkins University, and Dr. E. R. Weidlein, director of Mellon Institute of Industrial Research.

DR. EDWIN B. FRED, professor of bacteriology at the University of Wisconsin, vice-president of the Society of American Bacteriologists, was elected president of the society at the recent Baltimore meeting. He succeeds Dr. J. Edward Brown, associate professor of bacteriology at the Johns Hopkins University School of Medicine. Dr. William Mansfield Clark, DeLamar professor of physiological chemistry at the Johns Hopkins School of Medicine, was elected vice-president to succeed Dr. Fred. Dr. J. M. Sherman, of Cornell University, was reelected secretary-treasurer. Dr. Ivan C. Hall, of the University of Colorado, and Dr. Ralph R. Mellon, of the Institute of Pathology of the Western Pennsylvania Hospital, Pittsburgh, were elected members of the council.

DR. CHEVALIER JACKSON, of Philadelphia, was honored at the meeting of the Chicago Laryngological and Otological Society, Chicago Medical Society, Chicago Tuberculosis Society and the Chicago Tuberculosis Institute on January 4. An appreciation of the work of Dr. Jackson in diseases of the lungs was given by Dr. Ethan Allen Gray; in peroral endoscopy by Dr. Joseph C. Beck; in medical literature with the brush and pen by Dr. Morris Fishbein; as a fellow of the American Medical Association by Dr. Edward H. Cary, Dallas, Texas, and as a doctor of medicine by Dr. Frank Billings. Dr. Jackson addressed the meeting on "Suppurative Disease of the Lung Following Operations Involving the Upper Air Passages."

THE *Journal* of the American Medical Association writes editorially: "On January 2, the date of this issue of the *Journal,* Dr. George H. Simmons, editor and general manager emeritus of the American Medical Association, reaches his eightieth birthday. The felicitations of organized medicine are tendered to him on this occasion. From 1898 until 1924 he served the American Medical Association as a leader who brought it from a small body with practically no assets and an insignificant publication to a great organization, financially sound, with the greatest medical journal in the world, and with numerous bureaus, departments and councils that have exercised magnificent leadership in their special fields. In 1924 he retired and has spent his time since that date in travel and in recreation during the summer in Chicago and in the winter at his residence in Hollywood-by-the-Sea, Florida. The record of his achievements is written daily in the work of the organization that he so wisely builded."

DR. CHARLES NICOLLE, director of the Institut Pasteur of Tunis, has been nominated commander of the French Legion of Honor.

THE title of emeritus professor in the University of London has been conferred on the following: Dr. E. J. Garwood, on retirement from the Yates-Goldsmid chair of geology at University College; Dr. C. Spearman, on retirement from the chair of psychology at University College, and Mr. W. E. Dalby, on retirement from the chair of civil and mechanical engineering at the Imperial College—City and Guilds College.

AT the Mount Wilson Observatory, Dr. Joel Stebbins, director of the Washburn Observatory, University of Wisconsin, has been appointed research associate for collaboration in stellar photometric studies, and Dr. W. H. W. Baade, of the Hamburg Observatory, has been appointed astronomer.

MR. LEON CAMPBELL, who for many years has unofficially directed the scientific work of the American Association of Variable Star Observers at Harvard

Observatory, has been appointed the first Pickering Memorial astronomer. With increased facilities, he will have entire supervision of the observational work of the association, with headquarters at the Harvard Observatory. The "Pickering Memorial Fund" has been raised through the joint efforts of the American Association of Variable Star Observers and the Harvard Observatory to memorialize the work of Professor Edward C. Pickering. The income is to be used almost wholly to promote the study of variable stars.

PROFESSOR R. B. BRODE, of the department of physics of the University of California, will lecture at the Massachusetts Institute of Technology from February 6 to June 6.

A NEW pathological institute has recently been opened at the Sen Yatsen University in China, under the direction of Dr. Ernst Dormanns.

MR. JAMES A. BERRY, formerly of the department of bacteriology of the University of Washington, has been placed in charge of the microbiological studies on frozen-pack fruits and vegetables in the frozen-pack laboratory of the Bureau of Plant Industry at Seattle, Washington. This laboratory was established recently for the purpose of carrying on research and experimental work in all phases of preserving horticultural products by freezing.

DR. DAVENPORT HOOKER, of the University of Pittsburgh, was recently elected a member of the editorial board of *The Journal of Comparative Neurology.*

DR. F. HASTINGS SMYTH, who holds the doctorate of the Massachusetts Institute of Technology and was formerly a member of the scientific staff of the Geophysical Laboratory of the Carnegie Institution of Washington, was ordained a deacon in the Anglican Church on December 20. He will be attached to the staff of the Church of St. Martin, Brighton, England.

ACCORDING to *Nature,* Dr. H. R. Lang, acting secretary of the British Institute of Physics, has been appointed secretary of the institute and editor of the *Journal of Scientific Instruments.* Dr. Lang carried out research with the late Professor H. L. Callendar, and was afterwards demonstrator in physics at the Imperial College of Science and research fellow of the Institution of Petroleum Technologists.

DR. F. S. SINNATT has been appointed director of fuel research under the British Department of Scientific and Industrial Research. He has been assistant director since 1924. Previously he was lecturer on fuels in the University of Manchester faculty of technology, and director of research to the Lancashire and Cheshire Coal Research Association.

DR. ALBERT EINSTEIN arrived in California on December 29.

DR. KIRTLEY F. MATHER, professor of geology at Harvard University, has leave of absence for the second half of the year 1931–32.

Industrial and Engineering Chemistry reports that the Division of Chemistry and Chemical Technology of the National Research Council has recommended the following as delegates to the Madrid meeting of the International Union of Chemistry, April 3 to 10, 1932: *Members of the Council of the Union:* John Johnston, Charles L. Reese, Frank C. Whitmore, W. D. Bancroft (alternate for Claude S. Hudson), Edward Bartow (alternate for Charles L. Parsons), and H. E. Howe (alternate for James B. Conant). *Delegates:* Perry A. Bond, Hippolyte Gruener, J. R. M. Klotz, Henry G. Knight, Herbert R. Moody, Merle Randall, R. R. Sayers, Alexander Silverman and E. W. Washburn. The International Union of Chemistry will meet at the same time as the ninth International Congress of Pure and Applied Chemistry.

THE African expedition led by Professor and Mrs. Cockerell, and including Miss Alice Mackie and Mr. and Mrs. John Ogilvie, entered Africa at Lobito Bay in July and visited various places before reaching the Cape Province. The collections of insects are extensive and some mollusca were obtained. Miss Mackie took moving pictures and other photographs. Mr. and Mrs. Cockerell sailed from Cape Town at the end of November, leaving the others to continue the work in South Africa, probably for several months.

THE Field Museum of Natural History is sharing in a zoological expedition to French Indo-China, led by M. Jean Delacour, the French zoologist. The participation of the museum is financed by Mr. Marshall Field, of New York. The Paris Museum of Natural History and the British Museum (Natural History) will receive part of the collections. The expedition will return in May, 1932, after visiting little known regions in the province of Laos from Vientiane to Muong-Ting.

MR. LUCIUS S. STORRS, president of the United Railways and Electric Company of Baltimore, gave the second Aldred Lecture of the year at the Massachusetts Institute of Technology on January 8. He spoke on his "Engineering Experiences."

THE lectureship in science established several years ago by the Wisconsin Alumni Research Foundation of the University of Wisconsin was filled this year by Dr. Robert A. Millikan, of the California Institute of Technology. Dr. Millikan visited the university on January 5, 6 and 7. While there he conferred with faculty, graduate students and research workers in several departments, and gave several talks on research work. The title of his lecture on January 5 was "The March of Science." This is the third lecture

given under the auspices of the foundation, the other two lecturers being Dr. Walter B. Cannon, of the Harvard Medical School, and Dr. Ross Aiken Gortner, of the University of Minnesota.

PROFESSOR HENRY E. SIGERIST, director of the Institute of the History of Medicine in the University of Leipzig, gave a lecture at the University of Wisconsin on December 10 on "The Medicine of the Renaissance"; before the Chapter of Alpha Omega Alpha, of the University of Minnesota, on December 11 on "The Medicine of the Renaissance"; in the Mayo Foundation on December 14 and 15 on "The Development of Medicine during the Nineteenth Century," and "The Evolution of Medical Ethics"; before the Des Moines Academy of Medicine on December 16 on "The Medicine of the Renaissance: Fracastoro, Paracelsus, Veslaius, Pare," and at the University of Nebraska on December 17 on "The Medical Literature of the Early Middle Ages." Professor Sigerist attended the meeting of the American Historical Society in Minneapolis during the holidays and then went to San Francisco, where he will lecture on January 10. The arrangement of his itinerary in the Middle West was made by the Mayo Foundation.

THE executive committee of the second International Congress of Tropical Medicine announces that the congress, which was to be held in Amsterdam from September 12 to 17, has been postponed. Owing to economic conditions, the Dutch Government feels unable at present to grant its financial support, and private contributions so far received have been inadequate to cover the estimated expenses of the meeting. Subscription fees which have already been paid will be returned.

PROFESSOR E. G. CONKLIN, of the department of zoology of Princeton University, was reelected president of the board of trustees of the Bermuda Biological Station for Research, Incorporated, at the annual corporation meeting held in New York City on December 28. The other officers who were also reelected are Professor H. W. Rand, Harvard University, *secretary*, and Professor R. G. Harrison, Yale University, *treasurer*. Six members of the board of trustees were elected to serve until 1935. They are: Dr. E. J. Allen, director of the laboratory of the Marine Biological Association of Plymouth, England; Dr. E. V. Cowdry, Washington University, St. Louis; Professor E. Newton Harvey, Princeton University; Professor J. F. Fulton, Jr., Yale Medical School, and Dr. Wheeler and Professor Rand. Dr. J. F. G. Wheeler, of London, was appointed director of the station, which will be formally opened on January 6. A group of members of the society sailed on December 29 to be present at the opening at which the governor of Bermuda, Lieutenant General Sir Thomas Astley-Cubitt, will preside.

THE late Charles H. Tyler, who died on December 4, provided in his will for a bequest of $1,000,000 to establish and maintain a laboratory for surgical research at the Boston City Hospital. The laboratory is to be named for Dr. George G. Sears, professor of clinical medicine, *emeritus*, at the Harvard Medical School, who is a trustee of the hospital.

A RESEARCH committee of the Indiana University School of Medicine has been appointed, consisting of Mr. Hugh McK. Landon, of the Fletcher Savings and Trust Co.; Mr. Peter C. Riley, president of the Republic Creosoting Co., which has one of the best equipped research laboratories of the Middle West, and Mr. Eli Lilly, of Eli Lilly and Company, leaders in pharmaceutical research; Acting Dean W. D. Gatch, Dean Burton D. Myers and Professor R. E. Lyons, of the department of chemistry. The committee will control the income from $200,000 set aside by the will of Mrs. Hugh McK. Landon, the income from the Louis C. Huesmann memorial of about $65,000, and important annual gifts from Eli Lilly and Company. Grants will be made from these funds for promotion of well-defined research projects, a written statement of which must be presented to the committee.

BY the will of the late Mr. A. G. Weeks, of Marion, Massachusetts, his immense collection of butterflies of the world is given to the Museum of Comparative Zoology at Harvard University. With this is also bequeathed his entomological library and the sum of $100,000. This collection, long known as the finest butterfly collection in this country, was begun by Mr. Weeks when a young man by the purchase of the butterflies of Otto Poling. Later Mr. Weeks employed collectors to gather butterflies in South America, and published two volumes, beautifully illustrated, describing the new species, "Illustrations of Diurnal Lepidoptera Unknown to Science." In recent years he has added a large amount of valuable material both from collectors and from dealers. The collection is contained in forty-five fine cabinets.

THE general and local collections of herbarium specimens of the New York Botanical Garden have been enriched by gifts of herbarium specimens presented by Dr. Charles E. Moldenke, of Plainfield, New Jersey, and his son, Harold Moldenke, the latter a registered student at the garden. Mr. Kenneth K. Mackenzie, a member of the board of managers, has presented his private herbarium. It contains approximately 40,000 specimens, largely from North America, but representing also various other regions. This is the largest single accession received by the garden in recent years. It is supplementary to Mr.

Mackenzie's previous gift of his local material and undistributed duplicates. Mr. Raymond V. V. Miller, brother of the late Waldron DeWitt Miller, associate curator of birds at the American Museum of Natural History, has contributed the latter's private herbarium, consisting largely of plants collected in New Jersey. The collection contains between 2,500 and 3,000 specimens and forms an important accession to the local herbarium consisting of plants growing within 100 miles of the City of New York.

SCIENCE SERVICE reports that the Seventy-Second Congress now in session is expected to take favorable action on a bill establishing a national park in the Florida Everglades. The last congress considered such a bill, the Park Service of the Interior Department made a favorable report on the project and the bill passed the Senate, but failed in the House because it got caught in a legislative jam. This park would be part of a "comprehensive eastern park system" as viewed by the National Park Service. Great Smoky Mountains Park and the Mammoth Cave Park project, as well as the Shenandoah region of Virginia are among the others. The National Park Service is continually asked to investigate park projects, and has now on its docket for such investigation fifty-five national park and forty-four monument projects. Accomplishments of the past year listed by the National Park Service include the development of museums in parks and monuments, particularly the unique plan to emboss an actual fossilized skeleton of a dinosaur in his own home territory in the Dinosaur National Monument. The park and monument system increased its total area from 10,339,506 acres to 12,113,621 acres.

MR. STEPHEN C. SIMMS, director of the Field Museum of Natural History, reports that more than one and one-half million persons visited the museum during 1931. This marks a new record for a year's attendance, and makes the fifth consecutive year in which the one million figure has been exceeded. The previous record was made in 1930 when the attendance was 1,332,799, over which the 1931 total represents an increase of 182,664 or approximately 13½ per cent. In addition to the number of persons actually coming to the museum, as recorded in the attendance figures, more than 700,000 children have been reached through the extra-mural activities of the museum conducted through the N. W. Harris Public School Extension and the James Nelson and Anna Louise Raymond Foundation for Public School and Children's Lectures. This brings the total number directly reached by the museum's activities to well over two million. Beyond these, a still wider but numerically incalculable public is reached by the publications issued by the museum and circulated internationally, and by press reports, motion picture newsreels and radio lectures concerning the institution and its activities. Mr. Simms writes: "It is most gratifying to observe the constant and rapid increase in attendance at the museum, because it reflects the growth of public interest not only in the institution but in the sciences which the museum's exhibits serve to illustrate. It indicates that the museum is successfully fulfilling its mission as an educational factor in the life of our city."

A SCIENTIFIC reunion was held recently at the British Museum (Natural History), South Kensington, by the director and scientific staff, who displayed a number of exhibits selected from recent acquisitions. The London *Times* reports that a large sketch map of Central America and the adjacent island groups, prepared by Mr. P. R. Lowe, created much attention, since it was decorated with the bodies of different species of the West Indian Flower-pecker (*Coereba*) according to their distribution. Copies of photographs of the Eastern Gorilla, presented by the proprietors of the *Times*, and the photograph by Lady Broughton of a Tanganyika lion were among the exhibits of the Zoological Department. Another zoological exhibit was an unusual trout, from the River Coa in Portugal, which habitually grows upon its head a filamentous mass of sedge. The department of botany showed specimens from the Argentine-Chilean border lent by the King, while the department of minerals displayed photographs and specimens from the meteoric craters at Henbury, Central Australia, which were made known this year as the second largest meteoric craters in the world. A meteoric stone found by Mr. Bertram Thomas was also on view.

DISCUSSION

A THEORY OF DISINFECTION

DUCLAUX, in his "Traité de Microbiologie,"[1] (1898) devotes a volume or more to a consideration of enzymes. The bacterial cell, according to his views, carries on its activities entirely through its enzymes, the cell itself being relatively inert. In the final analysis, the properties of the cells are those of its enzymes, and by defining the latter we have a complete description of the former.

While such a point of view could hardly be accepted in its entirety to-day, it still provides a very satisfactory basis on which to explain the various phenomena connected with the disinfection process.

[1] E. Duclaux, *Traité de Microbiologie*, Masson et Cie, Paris, 1898.

Thus the apparent truth of Duclaux's proposition is revealed by such points as the following:

(1) Both enzymes and bacteria are destroyed by the same agents, *e.g.*, heat, light and chemicals. The effect of heat in each case becomes apparent at about 50 degrees Centigrade. Enzymes vary considerably in their response to chemicals, and no substance is known which is bactericidal and yet without action on any enzymes. Such agents as toluene are very destructive for certain bacterial enzymes. Mercuric chloride destroys the activity of enzymes but, on addition of a sulphide, the activity may be restored. Likewise bacteria "killed" by mercury may be restored to life by a similar treatment.

(2) In a dry condition, both enzymes and bacteria are highly resistant to heat.

(3) Both are resistant to cold to an astonishing degree.

(4) The rate of destruction when each is acted upon by an adverse agent follows that of a monomolecular reaction.

It will be seen that the phenomenon of disinfection may be explained on a no more complicated assumption than that the disinfecting agents act upon the enzymes of the bacterial cell in exactly the same way as if they were dispersed in solution. The cell itself need play no part; it is dead or alive at the end of a given exposure depending on whether or not it still contains a sufficient number of intact molecules of necessary enzymes. If a given species of organism contained in its make-up several different kinds of enzymes, one of which was very susceptible to the disinfecting agent, it would be this one which would determine the cell resistance.

It will be at once obvious from what we have said, that at the end of any given exposure three types of cells might be found in a suspension, *viz.*, cells which have not lost any enzyme, cells which have lost some, and cells entirely bereft of intact enzyme molecules. The last, of course, could not grow and would therefore be dead. One would expect a difference in the first two groups. In actual experimental work two types of survivors are found and while it is impossible to know the state of their enzyme molecules, their behavior accords very well with what one would expect from the enzyme theory. The two types actually found are:

(1) Organisms which appear not to have been affected by the disinfecting agent. They grow as well and as quickly as the untreated organisms.

(2) Organisms which develop only after a "lag,"[2] or, if tested on different media, are found to have become more particular in their nutritive requirements because of their exposure. The length of the lag period depends on the intensity and the duration of the treatment.

The types of survivors are well illustrated by some experiments of ours on an acid-fast organism.[2] This organism grew well on all solid media irrespective of the composition or the pH. It produced a large amount of alkali, bringing the medium on which it grew to pH 7.8. Practically the maximum number of colonies developed within 24 hours. After heating for one minute at 60° C. and plating, a few colonies developed within 24 hours, but the majority required 48 hours for development. When heated for 5 minutes no colonies appeared in 24 hours, a few appeared in 48 hours and many more in 72 hours. Finally, on heating for 20 minutes, no growth appeared in 72 hours and one colony developed in 96 hours. These tests were carried out with a medium of pH 6.6. It was found that a greater number of survivors could be obtained if the heated suspension were plated out on media of a pH close to the final one assumed by an unheated culture. Although the number of survivors was increased by such a medium the lag period was not affected. It will be seen that, provided the time of heating was short enough, a few organisms appeared to escape intact. Others were progressively injured as indicated by the lag before development or by their inability to grow on an acid medium.

In terms of the theory, our experiments would be explained as follows: A number, perhaps all, of the various enzymes in the make-up of the acid-fast organism were thermolabile at the temperature used. The application of heat progressively destroyed the enzymes so that at the end of a period of between one and five minutes no cells remained which had all of their enzyme intact. Some at this time still had enough enzyme to supply food so that more molecules of their kind could be formed, thus restoring the cell to its original condition. From 24 to 48 hours were required for this restoration. Other cells, which were more damaged, required longer periods. One type of enzyme molecule which was destroyed supplied alkali. This apparently was a more labile type of enzyme so that after certain heating periods there were cells totally lacking in this particular enzyme, yet having a sufficiency of all other necessary types. Such organisms recovered in an alkaline medium where the services of the enzyme in question were not so necessary.

The question may next be raised as to the resistance of two suspensions differing only in the average enzyme content of their cells. From the enzyme theory it would be evident that those cells having the greater numbers of enzyme molecules would be the more resistant. While such a case is difficult if

[2] M. L. Isaacs, "Factors with Influence Tests of Bacterial Survival, I and II," *Jour. Bacteriology*, Vol. XX, No. 3, Sept., 1930.

not impossible to find with certainty, in practice, we probably have an approach to it in cultures of a given organism at different ages. In an older culture competition for nutrition is keener and one might expect therefore the greatest enzyme content per cell. Experimental results indicate that old cells are in fact more resistant than young ones. In the case of spores the situation is different. The freshly formed spore contains its maximum supply of enzyme. Being in a resting condition it can generate no more, so that the effect of time would result only in a decrease of that already present. Experiments carried out by several workers indicate that old spores are less resistant than young ones.

There are other facts, besides those which we have considered, which may be explained in terms of the theory. Thus, the germicidal action of substances which lower surface tension may be pictured as due to a washing off of enzyme from the bacterial cell. Such also may be the effect of physiological saline solution. The problem of salt solutions is a complicated one, however. Differences in susceptibility depend on the kind of organism and the concentration and kind of surrounding ion.

It is a curious fact that Duclaux, although regarding enzymes as the basis of cell life, explained disinfection on a different theory. He assumed that in the case of heat, at least, a coagulation of the proteins of the bacterial cell was responsible for death. He based his theory on the observations of Marshal Ward on the *Bacillus rhamosus*. Ward had noted an apparent coagulation in the body of this organism when it was exposed to concentrated sunlight. A similar explanation of disinfection has been developed by Chick (1910) and more recently by Bancroft and Richter. Such a theory and the enzyme theory are by no means mutually exclusive, for most enzymes are either protein themselves or so closely associated with protein that they coagulate in the presence of those agents which react with proteins. The two theories differ chiefly in that one pictures the protoplasm of the organism as resistant, the other as labile.

The coagulation of the bacterial cell proteins affords in itself an adequate explanation of many of the facts observed in connection with disinfection. There are several phenomena, however, which are more easily accounted for by the theory of enzyme susceptibility. Some of these may be briefly mentioned. (1) Certain salt solutions such as physiological saline are immediately injurious or germicidal for some organisms under conditions which probably do not involve coagulation. (2) The results obtained with the acid-fast organism described above can hardly be explained on the basis of coagulation. (3) The greater resistance of young spores is difficult of interpretation in terms of coagulability. One would expect, if anything, a greater resistance in old spores.

M. L. ISAACS
COLLEGE OF PHYSICIANS AND SURGEONS,
COLUMBIA UNIVERSITY

THE COTTON ROOT ROT FUNGUS INDIGENOUS IN ARIZONA DESERTS

INVESTIGATORS who have been studying the cotton root-rot disease in Texas and Arizona have considered for several years that the fungus (*Phymatotrichum omnivorum*) was indigenous in the areas where the disease is prevalent. The occurrence of the fungus on the roots of certain native plants in the vicinity of cultivated areas, and the immediate appearance of the disease on susceptible crops, when planted on virgin lands, has been reported by Peltier, King and Sampson, Taubenhaus, Dana and Wolff, and by King and Loomis. At the root-rot conference at College Station, Texas, in January, 1931, Wolff reported the finding of infected wild plants and sclerotia on a railroad right of way that had not been under cultivation for forty or fifty years.

The writers have been studying the disease at Sacaton, Arizona, and have made it a practice on field trips through the desert to look for indications of the fungus on the native plants. Large areas of desert land are being brought into cultivation under several new irrigation projects in Arizona and California, so that it would be desirable to detect the presence of the disease before the expensive procedures of clearing, leveling and planting are undertaken.

On August 11, 1931, two of the writers observed extensive patches of spore mats of the root-rot fungus along the roadside in a desert area twelve miles north of Florence, Arizona, on U. S. Highway No. 80. These occurred intermittently over a distance of two miles on the vertical bank of the drainage channel made by the road grader in elevating the road bed. All the mats were located on the east side of the road, which was more shaded than the west, and which was still moist on the surface from recent rains. A profuse desert vegetation existed in this area consisting largely of *Covillea tridentata, Prosopis velutina, Franseria deltoidea, Opuntia fulgida, Condalia lycioides canescens, Sphaeralcea ambigua, Chamaesyce albomarginata, Aplopappus heterophyllus,* and various quick-maturing annuals. The nearest cultivated fields were about twelve miles distant, and the area, which was a high level plain near the mountains, was far separated from any of the general drainage water channels of the region.

On returning to the root-rot area three days later,

spore mats were found not only along the highway, but in several places on the shaded banks of a small wash for a distance of a fourth mile from the road. Other spore mats were found beneath the shade of mesquite and Condalia trees and Franseria plants. Several native plants were excavated, and on the roots of some of them were found typical strands of the Phymatotrichum fungus. The roots of three species, *Chamaesyce albomarginata, Aplopappus heterophyllus* and *Sphaeralcea ambigua,* showed evidence of definite injury by the fungus, but no lesions or rotted tissues were observed on the roots of *Prosopis velutina, Condalia lycioides canescens* and *Opuntia arbuscula,* on which the mycelium also occurred.

Samples of soil about one-half cubic foot in quantity were obtained from three areas near the patches of spore mats. These were washed in a set of sieves, and the coarser materials left on the sieves were examined for the presence of root-rot sclerotia. In one of the samples obtained from the roadside two sclerotia were found. When tested for viability in the laboratory one of them germinated.

This observation seems to afford definite evidence that the fungus is indigenous in virgin lands in the Southwest, and explains the occasional occurrence of the disease in the first cultivated crops that are planted after clearing the native vegetation.

C. J. KING
CLAUDE HOPE
E. D. EATON

U. S. DEPARTMENT OF AGRICULTURE,
WASHINGTON, D. C.

IS EVOLUTION ONLY A DETAIL IN THE DYNAMICS OF POPULATIONS?[1]

IN reading a recent copy of SCIENCE (June 26, 1931) I was much interested in certain rather naïvely dogmatic statements anent population growth, embodied in a paper by Professor W. H. Longley, entitled, "Evolution a Detail in the Dynamics of Populations."

It seems to me, without wishing on my own part to be too dogmatic, that certain of the statements in this article can be shown to be incorrect.

For example, the paper states that "the struggle for existence proceeds with an order as definitely predictable in its outcome as that manifested in a gaseous system under the terms of kinetic theory." It is my contention that such is the case if, and only if, the individuals of all the species concerned act as independent entities in the matter of decisions involving subsequent actions, in which case the unpredictable actions of individuals can be treated in terms of statistical "mean actions." If, on the other hand, any considerable group of individuals, whether all of one species or not, are sufficiently advanced mentally to "put their heads together" and make a group decision, the nature of this decision will not be *à priori* determinable, hence, if it has any effect on the population growth, the rate and nature of that growth will not be predictable on a mechanistic basis.

Reading further we encounter the statement: "but populations may not so increase [according to the logistic S curve, the brackets are mine] unless they are in fact sensitive to fractional increases in population pressure, as the formula requires." Is this not an example of the not uncommon fallacy of thinking that a "fitted equation" is necessarily a true expression of the real mechanism of a reaction? I can assert from actual experience with "mechanistic" mathematical analyses of insect population growths that an S-shaped curve can seldom be adequately explained on such simple grounds.

As to the further statement, that "it is astonishing, but a fact, nevertheless, that the measure of difficulty the generation of 1790 had in rearing its children in America should have gauged accurately the ultimate and largely unutilized resources of so great a country," it is certainly very surprising, if true, but I believe it can be shown to be incorrect on mathematical grounds.

It can further be shown by mathematical analysis that the exhaustion of resources is by no means the only factor capable of permanent alteration in a population curve. To state that the exhaustion of resources is such a sole factor is to say that if N be the number of individuals in a population at time T, and R the amount of as yet unutilized resources, then N is a function only of R, and so we may write $\frac{dN}{dR}$ as a total derivative, an hypothesis hardly likely to lead to any very complete insight into the mechanism of growth of a population.

Again a change in the form of activity of a population may do far more than change the limiting value; it may change the whole form of the curve, so that, instead of approaching some limiting value as an asymptote, it commences to oscillate between two limits, with constant amplitude. It may even exhibit a damped fluctuation.

It seems to me also that the statement that the effect of the varying rate of immigration into the United States has been nil is erroneous. It has certainly had an effect, but one would hardly expect to detect it by the rather crude method of comparison with a purely empirical curve, especially as the variations are at best small in comparison with the total growth in population from census to census.

[1] Published with the approval of the director as Miscellaneous Paper No. 10 of the Experiment Station of the Association of Hawaiian Pineapple Canners, University of Hawaii.

In conclusion, I would point out that evolution is only to be considered a negligible factor in the growth of populations of short duration, because its operations are slow. Surely, however, it must have had an effect upon the growths of the comparatively undisturbed populations of prehistoric days.

JOHN STANLEY

THE GROWTH OF STALACTITES

ANOTHER example of the formation of stalactites from the lime mortar used in a brick arch, under conditions similar to those described by Professor Ellis in SCIENCE for January 16, 1931, and by me in SCIENCE for April 10, 1931, has just come to my attention.

In sinking a well near Put-In-Bay, Ohio, on South Bass Island in Lake Erie, north of Sandusky, in 1897, the workmen broke into an unusually large vug lined with crystals of celestite. In 1901 a winding path for descending into the vug was constructed in order to make this vug available for exhibition purposes as the "Crystal Cave" and a brick arch was built over this passageway, both to give a more cave-like effect and to prevent surface material from washing in.

Surface waters, percolating slowly through the mortar joints of this arch, began the formation of stalactites immediately after its completion. This growth has been allowed to continue undisturbed for the past 30 years, until now the largest stalactites have attained a length of about six inches. Thus the rate of growth at Put-In-Bay is indicated quite accurately and, according to the data available for Fort Pickens, Fort Delaware, and Put-In-Bay, the rate of growth in all three instances is of the same order of magnitude.

The mainfall at Put-In-Bay is rather less than the rainfall at Fort Pickens and at Fort Delaware. It also appears that the rate of growth of stalactites at Put-In-Bay is somewhat less, but the relation between rainfall and rate of growth of stalactites in these cases may be no more than a coincidence. Without further information as to other factors it would be unsafe to assume that rainfall is the dominant factor in determining the relative rates of stalactite growth in these cases.

Stalactites and stalagmites, growing under purely natural conditions, are found in other caves in the vicinity of Put-In-Bay, but I have been unable to obtain information as to their rate of growth.

GRAGG RICHARDS

SPECIAL CORRESPONDENCE

FIELD ANTHROPOLOGY IN AUSTRALIA

FOR some years the Rockefeller Foundation has supported anthropological research in Australia. One of the chief centers of activity has been at the University of Adelaide, and during the last four years various expeditions have been undertaken by its board of anthropological research. The members of the most recent of these have just returned from Central Australia, where they have been successful in adding to our knowledge of the native Australian.

The locality chosen for this expedition was Cockatoo Creek, a spot about two hundred miles northwest of Alice Springs and about one hundred miles west of the geographical center of the continent. The site was beyond the country stocked with cattle; it was still occupied by a scattered population of more or less nomadic aborigines, entirely dependent for their subsistence on their own resources, unable to speak English, and in the majority of cases without having had any previous direct contact with Europeans.

Awaiting the expedition's arrival, a large number of natives, comprising chiefly members of the Ilpirra and Anmatjera tribes, but including a few Kukatja, Ngalia, and Walmala folk, had been assembled, and others arrived during their stay; in all, about one hundred and fifty individuals—men, women and children—were gathered together, having heard of our pacific intentions and being attracted by the novelty and by the promise of food in abundance. Amongst the tribes thus collected were members of one which only a few years ago had been responsible for the killing of one European and for attacks on others. Later, several reprisals had been taken by the police and a number of natives had been killed. To the expedition not the slightest sign of hostility was exhibited; the most cordial relations were established. The serious business of the scientific investigations was lightened by the good temper of the natives and leavened by their keen sense of humor. There was not the slightest suspicion that any malicious and magical use might be made, by members of the expedition, of the blood that was abstracted for blood-grouping, or of the samples of hair that were taken. They submitted with docility to tests that try the patience of Europeans.

Like previous expeditions undertaken by the University of Adelaide, teamwork was a feature of the one to Cockatoo Creek. Its personnel consisted of Dr. T. D. Campbell (organizer) and Mr. H. Gray, a student of medicine, whose work consisted of routine anthropometry, dental investigations, etc.; Professors J. B. Cleland and T. Harvey Johnston (blood-grouping, pathological conditions, etc.); Professor C. S.

Hicks and Dr. R. F. Matters (basal metabolism and physiological observations); Dr. R. H. Pulleine and Dr. H. K. Fry (observations on the special senses and psychological tests); Mr. N. B. Tindale (ethnologist to the S. A. Museum) made notes on the ceremonials, investigated each individual's tribal history, and so on; Mr. H. Hale (director of the S. A. Museum) was responsible for plaster casts; and photography, both cinematographic and still, was in the hands of Professor H. J. Wilkinson and Mr. O. Stocker.

As in previous expeditions undertaken by the board of anthropological research, the present one was carried out with the cooperation of the South Australian Museum. A considerable portion of the expense was borne by that institution and it took the opportunity of sending its taxidermist, Mr. A. Rau, with the expedition. The S. A. Museum secured a number of ethnological objects of considerable value, as well as a large number of natural history specimens.

The results of the expedition will be published from time to time in various channels as the work is completed. All that can be given at present is a very brief summary indicating the nature and scope of the information obtained. Particulars were then taken as to the individual's native name, tribe, class, totem, number of children, etc. Such details were obtained on ninety individuals. The native then passed on for routine measurements; these were carried out on thirty-three adults and comprised approximately fifty entries on cards for each person. Not only were standard measurements taken, but in addition, notes were made on the color of the skin, the eye, the hair, the condition of the teeth, etc. The hair tracts of twenty-four children were delineated. Special nasal measurements and notes on facial features were made on forty-nine adults. Physiological observations were obtained, on twenty-five individuals, of the rate of absorption of 0.9% sodium choloride by the subcutaneous tissues, and of the changes of pulse rate and body temperatures with atmospheric temperature. The spinal curvature of thirty-four adults was recorded diagrammatically, and at the same time similar records were taken of most of the Europeans temporarily present at Cockatoo Creek. Foot outlines were obtained of twenty-five grown-up natives and notes on the hands and feet of four. Records of palmar skin creases were made of forty-nine.

The individual then passed to be blood-grouped, the blood being obtained by puncturing an ear-lobe; ninety individuals were grouped, of whom sixty-four belonged to Group A, and twenty-six to Group O. In addition, a large number of cross tests were carried out between European red cells and native sera, and between native red cells and native sera. In all, the number of observations on blood-grouping amounted to approximately one thousand.

The basal metabolism was estimated on forty-two individuals (four tests on each) on whom were measured pulse rate, respiration rate, blood pressure, skin temperature and mouth temperature. The temperatures and pulse rates showed some interesting features. In the cold early morning, the mouth temperatures of natives were found to be as low as under 96°, and in some cases, the pulse rate was as low as forty-five per minute. The metabolic picture was computed by wet and dry bulb thermometer and kata thermometer readings throughout the day. Seven individuals had gum paper applied to the surface of the body with the object of ascertaining the surface area of the body—a long and unpleasant task, but one which was submitted to gracefully by the natives concerned. Other physiological observations included measurements of strength of the grip, of the back and of the legs, in sixty-six individuals. Pressure pain was measured on twenty subjects and thermal pain on ten. Visual acuity was tested on ten; forty-eight were tested on color blindness by means of Stilling's Tables. No case of color blindness was detected. Color discrimination was ascertained by Holgren's Wools, in twenty-one individuals, and this was checked by detailed observations of color nomenclature on thirteen. Seven observations of auditory acuity were made. Several attempts were made to determine the discrimination of warm surfaces at different temperatures, but no consistence could be obtained. The acuity of the sense of smell was tested on seventeen individuals; taste sensation was observed in five cases with solutions of sugar, salt and vinegar. Special perception was tested in the discrimination of numbers. Some native drawings were made on the structure of the eye, nose, and throat; observations were also made on the methods of technique in manufacturing string, weapons and utensils. General observations were made on social behavior and emotional expression.

Plaster casts were made of the faces of seventeen individuals, of the chests of three men and three women, of the hands of two persons and of the feet of two, and of body scars. Also two casts were secured of that interesting pathological condition, boomerang leg. Standard photographs (192 in number) were taken of most of the natives present, and, in addition, a large series of other still photographs as of special parts such as the eyes (15), the nose (12), and mouth, or pathological lesions (six instances), as well as incidents in the daily life of the natives and in their surroundings (forty-eight photographs). Similarly, cinematographic films were made, illustrating the daily occupations of the na-

tives, corroborees, the method of hunting suros, and the manufacture of utensils from the wood of the bean tree (*Erythrina vespertilio*). Altogether, approximately 273 official photographs were taken, and about 1,000 feet of large track (35 mm) cinema film, and 2,200 feet of small track (16 mm) cinema film, 200 feet being in color. In addition, individual members obtained a large number of photographs illustrating native life and the natural features of the country.

Observations and notes were made upon a series of corroborees which arose spontaneously during the expedition's visit. Notes were collected of the customs and behavior of the natives; a vocabulary of approximately three hundred words was obtained. The native names for various identified trees and shrubs were also collected. Only two or three cat's cradles were known by these natives. About a dozen phonograph records of songs were obtained.

J. B. CLELAND

SCIENTIFIC APPARATUS AND LABORATORY METHODS

CONCENTRATING PARAMECIUM AND ROTIFERS WITHOUT CENTRIFUGING

THE following methods have been developed and observations made while experimenting with a pure line of *Paramecium multimicronucleatum*. Rotifers will respond to the same concentrating methods in a similar manner, and it may well be that other protozoans and small metazoans will do likewise.

Infusions are prepared by boiling one gram of hay in 700 cc of distilled water for ten minutes. Such infusions are seeded with Paramecium on the second day, and are allowed to stand until a reasonably dense population of animals has developed. Usually this requires ten or more days.

Such cultures are then redistributed among glass containers of any size which have the approximate surface-to-volume ratio of an ordinary quart jar. No container should be more than half full of the infusion. Containers which have straight sides, while not necessary, are more desirable for this stage of the operation. To each of these containers is added an amount equal to the quantity of culture present of cooled infusion freshly made according to the formula given above.

The populations of these new infusions will be forced to congregate at the surfaces in bands on the sides of the containers during the next two or three days. From time to time they may be picked up with a fine pointed pipette and transferred to concentration tubes. The tubes used in these experiments were 30 centimeters long and had an internal bore of eight millimeters. Although the tubes used were of the dimensions indicated, it is probable that a considerable variation would not affect the result. While the collecting is in progress, excessive concentration in any one tube must be avoided, if all the animals collected are to remain alive. Each tube should have an air space of at least five centimeters at the top.

Final aggregation is brought about by shaking the concentrations in the tubes violently in such a manner that the bubble of air is forced to pass back and forth through the columns of infusions. If the tubes are then set aside in a vertical position, the organisms will settle to the bottom. The infusion above the aggregation of animals may be removed with a capillary pipette if a dense mass of protoplasm is desired, or it may be poured off if so complete a concentration is not needed.

The above method is efficient to the point that we have collected a volume of seven cubic centimeters of living Paramecium from five containers, each of two gallons capacity. The instructor who desires to concentrate a few thousand animals for laboratory demonstration will find that two quart jars will supply an ample crop for the most wasteful freshman laboratory.

There are several short cuts by which a concentration may be obtained more quickly if cultures in the proper condition are at hand. For example, the population of an old Paramecium culture can be forced to the surface by the addition of fresh infusion to it. This would save at least ten days. Again, if a middle-aged culture in which the animals have settled to the bottom of the culture is at hand, the animals can be concentrated by repeatedly drawing a long fine-pointed pipette through the débris on the bottom and emptying the material secured into concentration tubes. The hay settles to the bottom. The Paramecium will be forced out of it, and upward, until they reach the surface, if the tubes are allowed to stand undisturbed in a vertical position over night.

Usually an adequately concentrated supply of Paramecium for one or two laboratories can be secured directly from a culture by one of the methods described above, without the use of tubes. Such material can be very nicely kept in syracuse dishes. However, if one desires to supply a series of laboratories, it is advisable to use the tubes, as the Paramecium can be fed daily by shaking to force them to the bottom, pouring off the old infusion, and adding new.

EDGAR P. JONES

ZOOLOGICAL LABORATORY,
UNIVERSITY OF PITTSBURGH

A SIMPLE PHOTOMICROGRAPHIC APPARATUS FOR OBTAINING DEPTH OR FOCUS

THE generally prevailing use of the camera lucida as a means of illustrating Ostracoda, Foraminifera, insects and kindred small sub-spherical bodies evidences a difficulty commonly met with in achieving satisfactory photographic realization in combination with magnification. This is essentially due to the inability of the microscope's objective to provide depth of focus. The apparatus here described was constructed from the suggestions of Professor Albert Johannsen, of the University of Chicago. It furnished a magnification of from 12 to 28 diameters, depending upon the lens used, and was employed with excellent results in picturing the heads of flies and the wings and limbs of small insects. It proved satisfactory for the photographing of Ostracoda large enough to be retained by a forty-mesh screen, and offers the possibility of photographing even smaller ones.

The bellows of a good 5″ by 7″ view camera was extended by attaching to it an elongated wooden box, made by nailing together four boards, each eight inches wide by ten feet long. Cardboard diaphragms, properly sized, and containing circular openings four inches in diameter, were inserted into the box at distances of one third and two thirds of its length, respectively, to cut off any light reflected from its sides. The ground-glass plate and the plate-holder of the camera were then mounted in a cardboard support and fastened to one end of the tube, the seam being made light-proof by wrapping securely with black cloth. In a similar manner the bellows and lens of the camera were attached to the opposite end of the tube, and the whole apparatus firmly mounted upon a long table. Focusing was accomplished by means of the rack and pinion of the view camera. It required considerable care and precision, since very slight alterations of the distance between the lens and the object produce great changes in the focus on the ground-glass. Powerful illumination is essential, and two 250 candle-power bulbs, placed about eight inches from the object, were employed for this purpose. Under these conditions the time of exposure was between five and six minutes.

Since the size of the image obtained is a function of the length of the bellows extension, considerably higher magnification may be achieved by increasing the length of the wooden tube.

BRANDON H. GROVE

DEPARTMENT OF GEOLOGY,
THE UNIVERSITY OF CHICAGO

SPECIAL ARTICLES

CHEMICAL STUDIES ON TOAD POISONS, IV, BUFAGIN AND CINOBUFAGIN

DURING the past three years Dr. K. K. Chen and I have been studying toad poisons from both a chemical and pharmacological point of view. Due to the difficulty of obtaining somewhat large amounts of material the work has been rather slow. The isolation of the various principles from Ch'an Su, the dried venom of the Chinese toad, and from *Bufo marinus* has been reported[1]. A thorough pharmacological study of cinobufagin, isolated from Ch'an Su and of Abel's bufagin, isolated from *Bufo marinus*, undertaken by Dr. Chen, revealed that their physiological action is qualitatively about the same as that of the cardiac plant glucosides, but that there is a quantitative difference. The results of the pharmacological study of the principles isolated from Ch'an Su have been published[2]. These pharmacological findings make it appear very probable that the principles of toad poisons are chemically also very closely related to the cardiac aglucones of the plant kingdom. The reported formulae for cinobufagin $C_{29}H_{38}O_7$ (1c) and for Abel's bufagin $C_{28}H_{36}O_6$ (1d), however, do not indicate any close chemical relationship to the plant cardiac aglucones, which are C_{23} derivatives, as has been shown by Jacobs and coworkers[3] and by Windaus and coworkers[4]. Further investigation, and especially a careful analytical study of various derivatives of cinobufagin and bufagin, have led to the revision of the above formulae and the following ones are proposed, cinobufagin $C_{25}H_{32}O_6$ and bufagin $C_{24}H_{32}O_5$. The published analytical data check very well with these revised empirical formulae.

Cinobufagin, $C_{25}H_{32}O_6$, when treated with alkali, gives rise to a hydroxy-carboxylic acid with the opening of a lactone ring and the splitting off of acetic acid. On acetylation one acetyl group is introduced into the molecule. Under the influence of hydrochloric acid water and acetic acid are split off. On oxidation with chromic acid a mono-ketone is formed. From the foregoing one can conclude that the molecule of cinobufagin contains a lactone, an acetoxyl, a secondary hydroxy and a tertiary hydroxy group, thus accounting for the six oxygen atoms in the molecule. On catalytic reduction tetrahydrocinobufagin is

[1] (a) K. K. Chen and H. Jensen, *Proc. Soc. Exper. Biol. and Med.*, xxvi, 378, 1928–1929; (b) H. Jensen and K. K. Chen, *Jour. Biol. Chem.*, 82, 397, 1929; (c) *ibid.*, 87, 741, 1930; (d) *ibid.*, 87, 755, 1930.

[2] K. K. Chen, H. Jensen and A. Ling Chen, *J. Pharmacol. and Exper. Therap.*, 43, 13, 1931.

[3] W. A. Jacobs and E. L. Gustus, *Jour. Biol. Chem.*, 78, 573, 1928.

[4] A. Windaus, K. Westphal and Y. Stein, *Ber. d. deutsch. Chem. Gesell.*, 61, 1847, 1928; A. Windaus and Y. Stein, *ibid.*, 61, 2436, 1928.

formed, thus indicating the unsaturated nature of cinobufagin.

Bufagin, $C_{24}H_{32}O_5$, when treated with alkali, forms a hydroxy-carboxylic acid with the opening of a lactone ring and splitting off of formic acid. On acetylation one acetyl group is introduced. Under the influence of hydrochloric acid formic acid and one molecule of water are split off. Several attempts to secure a ketone by oxidation with chromic acid have failed. From these findings one may conclude that the molecule of bufagin contains a lactone, a formoyl and a tertiary hydroxy group, thus accounting for the five oxygen atoms in the molecule. On catalytic reduction tetrahydrobufagin is formed, indicating that bufagin is also of unsaturated nature. Catalytic reduction of cinobufagin and bufagin also gives by-products of acid character, probably formed by the opening of the lactone ring.

From the foregoing one can see that the chemical behavior of cinobufagin and of bufagin is quite similar to that of the plant cardiac aglucones. By splitting off the acid radical which is attached to a hydroxy group (acetic acid from cinobufagin and formic acid from bufagin), one obtains compounds which are C_{23} derivatives, as are the aglucones of the plant glucosides.

$$\underset{\text{Cinobufagin}}{C_{26}H_{32}O_6} \xrightarrow{+H_2O} C_{23}H_{30}O_5 + CH_3COOH$$

$$\underset{\text{Bufagin}}{C_{24}H_{32}O_5} \xrightarrow{+H_2O} C_{23}H_{32}O_4 + HCOOH$$

While the cardiac poisons of the plant kingdom are combined with carbohydrates these principles of toad poisons are coupled with acetic acid or formic acid, or, as in the case of the nitrogen containing principles, which also have a cardiac action, with suberylarginine and an acid radical.

It has already been mentioned that bufagin under the influence of hydrochloric acid will lose formic acid and one molecule of water.

$$\underset{\text{Bufagin}}{C_{24}H_{32}O_5} \xrightarrow{HCl} \underset{\text{Bufagien}}{C_{23}H_{30}O_3} + HCOOH + H_2O$$

Bufagin is a mono-hydroxy lactone containing three double bonds and should give on catalytic reduction a compound of the composition $C_{23}H_{36}O_3$. It is hoped that the latter substance may be identical with one of the known reduced anhydro compounds of the cardiac aglucones of the plant glucosides[5]. If this should be true it would be a direct proof of the chemical relationship between these two groups of natural compounds.

[5] See reference 4; W. A. Jacobs and A. M. Collins, *Jour. Biol. Chem.*, 63, 123, 1925; W. A. Jacobs, R. C. Elderfield, A. Hoffman and Th. B. Grave, *J. Biol. Chem.*, 93, 127, 1931.

This reaction is now being carried out and attempts are also being made to convert cinobufagin by similar steps into the corresponding compound. The results so far obtained and those which are being accumulated will be published later in greater detail.

Another method of approach to show the possible chemical relationship between the cardiac principles of toad poisons and those of the plants may be as follows: Bufagin and cinobufagin are converted with alkali into the corresponding hydroxy-acids and these are then catalytically reduced, giving C_{23} derivatives, which may be identical with compounds obtained from the plant aglucones in a similar manner. This method is being investigated.

The author wishes to express his thanks to Dr. K. K. Chen of the Lilly Research Laboratories, Indianapolis, for the supply of Ch'an Su and the secretion of *Bufo marinus*. The author is also indebted to Mr. Hutchison of the Antivenin Institute, Glenolden, Pennsylvania, for an additional supply of the secretion of *Bufo marinus*. The research has also been aided in part by a grant from the Committee on Scientific Research of the American Medical Association.

H. JENSEN

THE JOHNS HOPKINS
MEDICAL SCHOOL

CRYOLYSIS OF LYOPHILIC COLLOIDS, AND ITS BEARING ON THE MECHANISM OF ENZYME ACTION

IN a recent publication[1] evidence has been furnished that the surface tension of a solution containing lyophilic biocolloids is increased by ethylene as well as by acetylene. At the same time the observation was made that the viscosity of the same solutions was decreased. These observations were interpreted on the basis that ethylene or acetylene are in a state of physical adsorption on the surface of the lyophilic colloids. The gases exert accordingly a protector action on the combined carrier-enzyme surface, the latter being increased when submitted for shorter or longer time to the influence of freezing.[2] In order to obtain a detailed information of the latter observation solutions of different concentration of egg albumin, gelatin, gum arabic and sodium oleate were investigated by means of cryolysis. The measurements of the surface tension of all the solutions in all concentrations, having been frozen once or repeatedly at different temperatures, furnished a decided increase. The viscosity, however, was decidedly increased solely in the case of egg albumin, decreased when using solutions of gelatin or gum arabic and immaterially influenced in the case of sodium oleate solutions. The electrical conductivity

[1] F. F. Nord, *Trans. Faraday Soc.*, 26: 760. 1930.
[2] F. F. Nord, *Nature*, 120: 82. 1927.

of solutions in the range of a concentration of 0.01 per cent. was decidedly increased, whereas in a concentration above 0.1 per cent. it was decreased, and finally the speed of cataphoretic mobility of the colloid particles, when measured in solutions of gelatin and egg albumin, was practically doubled after freezing.

The fact that the values of viscosity determinations were decreased in some cases, but the values of surface tensions, however, showed a thoroughgoing increase, may be understood if we recall that, for instance, a zymase solution or a gelatin solution may be considered as in every respect a more heterogeneous system, when compared with a solution, for instance, of egg albumin. It is necessary, therefore, to assume that under the effect of freezing, lyophilic colloids not only may undergo a disaggregation of the colloid particles, but also an aggregation of the same.

In so far as the values of surface tension measurements are concerned, we noticed that the reapproachment to the value of the surface tension of conductivity water was the greater, the more the respective solutions were diluted. In order to understand this fact we may assume, in accordance with the considerations of Gortner and coworkers[3] concerning the bound ⇆ free water equilibrium, that the surface tension of a lyophilic colloid solution is a resultant of three factors. namely, (a) the surface tension of colloid particles, and (b) the surface tension of the aforementioned two parts of the water system. Under the assumption that the increase of the value of surface tension of a frozen and afterwards thawed solution of lyophilic colloids, if not caused by dehydration of the particles, is due to a decrease of the radius of the colloid particles and subsequent increase of the surface of the same, then the bound ⇆ free water equilibrium is shifted in disfavor of the free water component. Accordingly, the surface tension increases only to the extent of the difference between the surface tension value of the particles and the value of surface tension of the water system. The diluted solutions on the other hand contain less particles, and accordingly the additional amount of bound water, when the surfaces are increased on account of freezing, as well as the difference between the value of the particles and the value of water, is smaller. The increase of the value of the surface tension is therefore greater in the case of diluted solutions. It was noticeable in this connection that sodium oleate solutions showed a decided diminution of volume in frozen state, compared to the unchanged volume of frozen proteins or gum arabic. This observation may be due to the possibly much less material rôle of the bound water in the case of colloidal electrolytes.

[3] R. A. Gortner, "Outlines of Biochemistry," p. 230, New York, 1929.

The disaggregation-aggregation hypothesis is supported also by the results of the measurements of conductivity and cataphoretic mobility of the particles, since we assume that the conductivity of dispersoid systems, containing the same amount of substance in dispersion of different degree, is reciprocally related to the square of the radius of the particles. The conductivity itself is based on the particles of highest dispersion of the system. The increased cataphoretic mobility on the other hand may be due to the fact that on account of increased dispersion, electrolytes went into solution out of the proteins, causing an electrical charge of the particles, which again causes an increase in their mobility.

Since, according to our present knowledge, proteins have to be considered not only as the substance of crystalline enzymes, but also as being responsible for the properties of the so-called carriers, so we may consider the results of our measurements as the first exact proof of the colloidal carrier's dependence of functioning on its physical state. At the same time it may be also mentioned that the biochemical behavior of physically transformed lyophilic colloids is distinctly different from the original systems.

It is intended to report in detail the experiments in the *Kolloid Zeibschrift.*

F. F. NORD
O. M. VON RANKE-ABONYI
PHYSIOLOGICAL INSTITUTE
TIERÄRZTLICHE HOCHSCHULE
BERLIN, NW 6.
OCTOBER, 1931

SEX DIFFERENCES IN THE STUDY OF GENERAL SCIENCE

THE purpose of this study is to investigate sex differences in the difficulty of the course in ninth-grade general science, and variations in difficulty from one part of the course to another.

Since competent writers of general science text-books do not agree upon what to include in the course, the selection of material for investigation must necessarily be arbitrary. Seven hundred forty-eight statements were chosen from text-books commonly adopted, the intention being to obtain a varied and random sampling of material sometimes used. To validate the use of these statements, ten text-books were surveyed to discover the extent to which these statements were incorporated into the course. A statement was counted as being in a book if found stated directly or if material was found from which it could be readily deduced. Table I summarizes these findings.

This list of statements agrees as well with the text-books as they do with each other and is better justified than a list of statements found in all text-books, since

TABLE I

Author	Text	Edition	Statements found
Hessler	First Year of Science	1925	615
Hunter and Whitman	Problems in General Science	1930	612
Van Buskirk and Smith	Science of Everyday Life	1928	598
Wood and Carpenter	Our Environment—How We Use and Control It	1927	567
Caldwell and Curtiss	Introduction to Science	1929	566
Webb and Didcoct	Early Steps in Science	1924	552
Snyder	General Science	1925	532
Pieper and Beauchamp	Everyday Problems in Science	1925	508
Hodgdon	Elementary General Science	1928	442
Clark	New Introduction to Science	1928	364

only 150 of the 748 statements were used by every author. Text-book writers agree least on plant and animal biology, on biographical material and on foods and clothing. They agree best on heat, electricity and weather. Some authors omit entirely large bodies of material included by most of the others.

supplied the material used for calculating the difficulty of the questions. The tests were given at the end of the year, after a period of review, by the class teachers under uniform conditions. The papers were scored uniformly by the experimenter.

Since the content of general science is not standardized, the questions were classified in terms of the science from which they were originally derived. An adequate classification is difficult because a single statement may be used in any one of several contexts. For instance, carbon dioxide is discussed under atmosphere, chemistry, foods, fire extinguishers, respiration, ventilation, oxidation and geology. Questions were finally classified on the basis of their occurrence in other secondary science books, as physics, chemistry, physiography, etc.

The per cent. of pupils who missed each question was calculated separately for boys and girls. For each question the per cent. of boys missing the question was subtracted from the per cent. of girls missing the question. That is, a question missed by 84 per cent. of girls and 76 per cent. of boys would yield a *difference* of plus 8, indicating the question is easier for boys than for girls. Minus differences indicate the questions are easier for girls.

Table II shows the distribution of the 748 questions arranged in order of the difference in difficulty for

TABLE II

DISTRIBUTION OF DIFFERENCES IN DIFFICULTY FOR BOYS AND GIRLS

Minus class intervals										
34–30	29–25	24–20	19–15	14–10	9–5	4–0	0			
N1	1	2	7	11	42	107	14			
Plus class intervals										
0–4	4–9	10–14	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54
N187	170	78	58	31	18	8	7	5	0	1

The statements were converted into completion or recall questions. C. I. Hull (Aptitudes Testing, New York, World Book Co. Page 313) reports that the self correlation of various forms of tests is: true-false r equals .507, multiple-choice r equals .556, completion r equals .618. Completion also lessens the chance of neighboring questions affecting the test item by eliminating needless words used as alternates in multiple-choice questions. Completion questions can not be answered by eliminating the wrong answers.

Subjects tested were chosen by the selected sampling method from six widely scattered states, all in the North except Virginia. School enrolments ranged from less than one hundred to more than two thousand in a single building. Farming communities and one select city residential district supplied the pupils tested. Three hundred boys and three hundred girls

boys and girls. Only 14 questions were equally difficult for both, 187 were from 0 to 4.9 per cent. easier for boys, etc. The median question in this distribution has a difficulty difference of plus 5.06.

Table III shows the median difficulty of the questions when arranged in groups. Of the twelve questions on clothing, the median question was 3.3 per cent. points easier for girls than for boys. Read downward the topics become increasingly difficult for girls and increasingly easy for boys, relatively.

The differences are significant when the median is three times its probable error. Only three subdivisions of material fail to show significant differences, while thirteen of eighteen favor boys.

The mean of the 748 differences is 6.1 per cent. points, counting as minus differences favoring girls.

Typical questions on which girls excel are: Artifi-

TABLE III

MEDIAN DIFFICULTY OF QUESTIONS UNDER SUBDIVISIONS

Type of material	Number of questions	Median difference	P.E.md
Clothing	12	− 3.3	.5
Food	27	− 2.8	.9
Earth, weather, geology	79	.1	.4
Human biology, health	60	.6	.5
Life processes and development	20	1.9	.7
Properties of matter	8	2.5	.8
Plant and animal biology	49	2.8	.7
Biography	34	2.9	.6
Astronomy	19	3.2	.8
Light	51	4.0	.8
Chemistry, air, water, matter fuels	115	4.4	.4
Heat	48	6.0	.7
Simple machines, work, laws of mechanics	50	6.4	.9
Sound	19	9.9	1.8
Measurement, introductory method	19	11.2	1.6
Fluids, barometers, pumps	32	11.9	1.1
Electricity	68	12.0	.9
Applied mechanics, engines, etc.	38	22.2	1.3

cial silk made from wood pulp is called rayon; iodine mixed with starch gives a dark blue color; a disk one third red and two thirds yellow appears to be orange when rotated rapidly.

Boys are superior to girls in answering these: Lamp cords are insulated with cloth and rubber; a dry cell is contained in a zinc can; an airplane with two sets of wings is a biplane.

Apparently a large part of the differences can be explained by environmental factors differing in the experience of boys and girls. These factors may or may not explain the much greater interest of boys in mechanics. At any rate, sitting in the same classes, reading the same text-books, receiving instruction from the same teachers do not operate effectively in eliminating sex differences.

This study brings out more questions than it solves. Should we shift emphasis on the course for boys and for girls to attempt to equalize their achievement? Is one course over mechanics adequate for a girl who must live in houses and offices dominated by machines? Do sex differences carry over to teachers of general science? What advanced sciences should be provided for girls, since they are so obviously handicapped in physics? Should boys and girls be taught separately since their interest as manifested in achievement is considerably different? Should boys and girls be graded on the same distribution?

CONCLUSIONS

General science is far from standardized in textbook treatment, the range of completeness being from 48 per cent. to 82 per cent. of a random checklist of items.

Material in general science is not arranged in either uniform or logical manner.

Material generally covered in biology shows no sex difference in general, but the subdivisions show slight differences.

Physiography material shows no sex differences.

Chemistry material shows significant differences in favor of boys.

Physics material is much easier for boys than for girls, the median differences on subtopics ranging from 4 to 22 per cent.

The subject as a whole is much easier for boys than for girls.

VICTOR C. SMITH

WASHBURN HIGH SCHOOL,
MINNEAPOLIS, MINN.

PROGRESS IN WESTERN DUCK SICKNESS STUDIES

A RECITAL of the events and experimental work that have led to a new concept of western duck sickness, formerly attributed to the toxic action of certain soluble salts of "alkali," was published in SCIENCE late in 1930.[1] The findings there chronicled strongly suggested that a type of botulism was involved. A supplementary contribution by Drs. Giltner and Couch,[2] appearing at the same time, set forth the fact that the bacillus of *Clostridium botulinum*, Type C, was present in mud obtained at Tule Lake, California, as well as in the tissues of wild mallards, pintails and ring-billed gulls that had died of or were killed while afflicted with duck sickness.

Despite the highly suggestive character of these circumstances and other facts revealed, including the striking similarity of the syndrome of duck sickness and that of botulism as manifested in "limberneck" of domestic ducks, there still remained the necessity of demonstrating actual toxin production under field conditions, and in media or associated with food or water likely to be ingested by birds. With an organism as prevalent and widely distributed as *Clostridium botulinum*, the need for such added evidence is obvious. As expressed by Geiger, Dickson and Meyer,[3] "the

[1] E. R. Kalmbach, "Western Duck Sickness Produced Experimentally." SCIENCE, lxxii, 1878, 658–659, Dec. 26, 1930.

[2] L. T. Giltner, J. F. Couch, "Western Duck Sickness and Botulism." *Ibid.*, 660.

[3] J. C. Geiger, E. C. Dickson and K. F. Meyer, "The Epidemiology of Botulism," U. S. Public Health Bulletin No. 127, pp. 1–119, September, 1922.

mere isolation of this organism from the suspected feed or intestinal tract or spleen of a dead animal is insufficient to support the diagnosis of botulism." There is always the likelihood that tissues may be invaded after death by this saprophytic anaerobe which, in the form of a few adventitious spores, may have been making harmless passage through the alimentary tract at the time of the creature's death, brought about by factors that may have been wholly foreign to botulism.

Demonstration of toxin in the field, therefore, in concentrations lethal to birds and in, or closely associated with, likely food items, constituted the primary objective of the past season's study of duck sickness conducted by the Biological Survey in southern Oregon and northern California. It was the logical sequel to the progress made in 1929 and 1930, and it constituted the final unforged link in the complete chain of evidence. Without such toxin demonstration the concept of botulism being the cause of duck sickness still would remain, at least to the bacteriological world, an unproved theory.

It is gratifying to be able to announce that this primary objective was attained, not once, but in at least twenty different instances during the summer of 1931. In each case toxin was demonstrated by the feeding or inoculation of experimental birds (mainly pigeons) with material obtained directly from field sources. Necessary toxin-antitoxin tests were made on every occasion, definitely identifying the toxin as that originating from *C. botulinum*, Type C, of Bengtson.

The media in which toxin was demonstrated included the bodies of birds dying of duck sickness, mud in the immediate vicinity of such bodies, water from shallow pools in infected areas, living and dead larvae of sarcophagid flies, submerged barley and other grains that had lain on mud flats where they were subjected to high temperatures and an alkaline environment. Under conditions that were strictly experimental but which might easily occur in the field, toxin also was demonstrated in a mixed mass of insect remains, copepods, snails, algae and Lemna. The organism, but not its toxin, was also demonstrated in such apparently suitable toxin-producing media as the dead larvae and pupae of hydrophilid beetles, submerged wheat heads of the season's crop, colonies of dead copepods, and miscellaneous insect débris, drifted ashore by wave and wind action.

This array of demonstrated toxin-producing media, though admittedly only a beginning, already includes food items that would indicate the channels through which ducks and shore birds may contract the trouble. Added research we feel is destined to extend the categories of toxin-producing food items of these two groups of highly susceptible birds and, bit by bit, round out our knowledge of the sources and vehicles for the toxin now known to affect more than sixty species of wild birds.

During 1931, progress also was made in the study of other aspects of this problem. Mentioning only a few, attention may be called to the apparent and doubtless important correlation between the incidence of duck sickness and alkalinity; the presence of Type C to the almost if not total exclusion of other types of botulism in duck sickness environments; the degree of susceptibility of lower organisms, both vertebrate and invertebrate, the death of which in the course of an outbreak of the disease would greatly augment the quantity of toxin-producing media; the toxin-destroying properties of high concentrations of certain alkaline salts and the possibility of thus explaining the absence of duck sickness from certain areas otherwise suited to its occurrence. These and numerous other aspects of the problem, mainly bacteriological in character, bid fair to make future studies of duck sickness of as great importance to the bacteriologist as to the conservationist of wild life.

During the season of 1931 Mr. M. F. Gunderson, employed by the Disease Investigation Project of the Biological Survey, worked with the writer, and to him is due great credit for the progress made along bacteriological and toxicological lines. At present he is continuing these studies in the department of bacteriology and immunology, University of Minnesota. It is a fruitful field for pure research as well as for the attainment of results of great practical value in the preservation of wild birds, and it stands to reason that, before the story of western duck sickness is completed, earlier concepts of the disease as well as the prevalence, range and economic importance of *Clostridium botulinum*, Type C, will be greatly changed.

E. R. KALMBACH

BIOLOGICAL SURVEY,
U. S. DEPARTMENT OF AGRICULTURE

BOOKS RECEIVED

Annual Report of the Board of Regents of The Smithsonian Institution. Pp. xii + 650. Illustrated. United States Government Printing Office. $2.00.

BROWNING, ETHEL. *The Vitamins.* Pp. xxxii + 575. Williams and Wilkins.

GOODENOUGH, FLORENCE L. *Anger in Young Children.* Pp. xiii + 278. University of Minnesota Press. $2.50.

GRAY, HORACE and J. G. AYRES. *Growth in Private School Children.* Pp. xv + 282. University of Chicago Press. $3.50.

International Address Book of Botanists. Pp. xv + 605. Braillière, Tindall and Cox. London. 12s. 6d.

RORIMER, JAMES J. *Ultra-violet Rays and Works of Art.* Pp. xii + 61. 47 plates. The Metropolitan Museum of Art. $2.00.

SCHAIRER, JOHN F. *The Minerals of Connecticut.* Pp. 121 + vii. State Geological and Natural History Survey.